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THE PSYCHOLOGICAL REVIEW

DID FECHNER MEASURE '*INTROSPECTIONAL*' SENSATIONS?

BY H. M. JOHNSON

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The objective psychologist has been charged with inconsistency in his attitude toward the doctrine known as psychophysics. On the one hand, he makes a very free use of the *methods* of psychophysics, having found them, indeed, to be indispensable to his business. On the other hand, if he has given much thought to the question, he is very likely to join Watson (**20**) in affirming that the *problems* of psychophysics, which led Fechner (**8**, i) and his followers to develop the methods, are spurious, inasmuch as the operations which are necessary to the solution are impossible of performance. Both Watson and Fechner rest their arguments on the validity of their own proclamations, leaving in dispute a point in scientific method which is exceedingly interesting of itself, and fundamental to psychology as an organized field of study. The time seems ripe, therefore, for the presentation of a new critique of the argument for psychophysics as a possible science, along with an objectivistic interpretation of psychophysical procedure.

The question turns chiefly on the character of Fechner's interpretative statement of Weber's law, as expressed in his fundamental formula

$$d\gamma = K\frac{d\beta}{\beta}, \qquad [1]$$

and more specifically, whether the formula is empirical or

assumptional. The latter interpretation is implicit in my recent and incidental allusion (14) to Fechner's system as being founded on an equivocation. The former interpretation is directly asserted by Professor Boring (2), and forms the basis of his criticism of me. Of the fundamental formula [1] Boring says:

"We have here two observed terms in observed correlation: $d\beta$ is that observed increment of stimulus that is observed as the correlate of $d\gamma$, an observed increment of sensation, the just noticeable difference. . . . The entire argument rests upon the empirical fact . . . that such differences . . . may be even observable as just noticeable."

It is indeed true, as Boring elsewhere remarks, that Fechner *called* this formula strictly empirical; and it is also true that he often mentioned 'the just noticeable difference in sensation' *as if* it were a datum of direct observation. But the question is one of fact, and it does not lend itself to settlement by the criterion *ipse dixit.* It is necessary to examine the procedure, no less than the assumptions, which lie back of the *dictum.* I should have preferred that Dr. Boring present such an analysis rather than merely stating the question at issue; but in the circumstances it seems as well to present the results of my own. They may be summarized as follows:

1. Fechner's fundamental formula, together with the formulas of measurement (the *Massformel* and its consequences), is not empirical but definitive.

2. The judgment of a *just noticeable difference in sensation,* symbolized by $d\gamma$ in the fundamental formula [1], is not reached by direct comparison of two introspectional sensations, but by means of *a census of classified responses;* the classification and the enumeration being performed, not by the subject of the experiment, but by the experimenter.

3. The Fechnerian sensation (the intensity of which is defined by the well-known *Massformel,* obtained from integration of [1]) has important properties that are incongruous with those belonging to introspectional sensations. Hence, the identification of Fechnerian sensations with introspec-

tional sensations, though usually taken for granted, is illicit. Pending the development of this proposition, we shall use two different terms to designate the two entities.

Before proceeding to the defense of the above theses, we must indicate a distinction that is essential to all which follows. I refer to the distinction between an empirical relationship and a defining equation. For illustration, we may borrow two examples from any good elementary text-book of old-fashioned physics.

Consider first a simple pendulum, or one which we may permissibly treat as if it were actually simple. Let us employ the symbol l to denote its length, the symbol P to denote its period (*i.e.*, the time required for one complete oscillation), and the symbol g to denote the gravitational constant of acceleration for the locality in which the pendulum is to be studied. All of these variables are empirical, and each of them can be measured without reference to any of the others. For instance, we can obtain the length l by direct application of a finely divided standard scale, such as a meter-stick. Similarly, we may measure the period P by counting the number n of oscillations which the pendulum executes in T seconds, while it swings through a small arc, and taking $P \equiv T/n$ seconds. Likewise, we may ascertain the value of the gravitational constant g with great accuracy, by making use of a modern substitute for Atwood's machine.[1]

If these independent determinations are carefully made, it will be found that whatever the absolute values of l, P and g may be, the following relationship is satisfied:

$$P^2 g/l \approx 4\pi^2 \qquad [2]$$

in which the symbol $\approx$ is to be read, "*varies quite closely about*" . . . or "*very nearly equals.*" This relationship is strictly empirical, in that it has been *observed* to hold

[1] Two metallic plates are connected to opposite poles of a spark-coil, the rate of sparking being controlled by a tuning-fork or other oscillator of high and constant frequency. A metal ball is allowed to fall *freely* between the two plates (in a vacuum if one prefers), and thus bridge the gap between them. On a strip of paper laid over one of the plates, the ball burns a record of its transit in the form of a succession of sparks, equally separated in time, and measurably separated in space.

amongst the three *observed terms*. By way of contrast, consider next the well-known expression

$$\mathrm{E} \equiv \tfrac{1}{2}MV^2 \qquad [3]$$

which exhibits the kinetic energy E of a body as a function of its mass M and velocity V. The latter expression is not an empirical relationship, but a formal definition. Kinetic energy is a purely formal entity: an arbitrarily selected function of mass and velocity, as defined above. The defining equation [3] is properly written as an identity, not an equivalence.

Another important distinction is bound up with the one just mentioned, and most unfortunately obscured in Fechner's treatment. If any two of the variable terms in equation [2] are given, we may use them as a *test*, or a means of *indirectly determining* the unknown. For example, we may take

$$\mathrm{l} \equiv P^2g/4\pi^2,$$
$$\mathrm{P} \equiv 2\pi\sqrt{l/g},$$

and

$$\mathrm{g} \equiv 4\pi^2l/P^2$$

or, in other words, *define* each of the variables as a function of the other two. If we should do so, however, we shall find that in each of the three cases, *if we make the measurements and perform the operations indicated in the right member of the defining equation, the result, expressed in the left member, will agree with the result of an* independent *measurement of the unknown variable, within a determinable and tolerable error: i.e.*, $\mathrm{l} \approx l$; $\mathrm{P} \approx P$; *and* $\mathrm{g} \approx g$. Every genuinely empirical equation will satisfy this condition.

It is not so with respect to equation [3]. If we make the measurements and perform the operations indicated in its right member, the result, E, expresses nothing that can be derived independently of the procedure which the equation prescribes. We might, for example, set $\mathrm{E} \equiv F \cdot s$, the product of the force and the distance through which it acts, but when we express $F \cdot s$ in terms of fundamental dimensions, we find that it is merely $\frac{1}{2}MV^2$ written in another way. Similarly if we convert kinetic energy into heat and exhibit the thermal

equivalent of E: we find that we are now considering the energy of motion imparted to another set of bodies and are expressing it in terms of their masses and their changed velocities, just as before. We may say, not only that E is determined by $\frac{1}{2}MV^2$, but that the determination is *absolute, perfect, and unique.* It would be very confusing to say that E is *indirectly* determined by $\frac{1}{2}MV^2$, for that would imply that another mode of determination exists, which would properly be called *direct.* Such an implication is contrary to fact.

As I have just indicated, Fechner introduces just this confusion, by treating a fact of definition as a datum derived by indirect measurement. In (**8**, Vol. i, p. 45) he defines Sensitivity (*Empfindlichkeit*) as the reciprocal of that magnitude of a stimulus-difference which has a constant (*i.e.*, liminal) effect on the subject. The result of the definition he terms an *indirect measure.* In keeping with his usual painstaking procedure in exposition, he resorts to amplification and illustration to insure that his identification is understood. He likens the procedure to the one employed in the determination of *Kraft*[2] which (so he tells us) is *indirectly measured* in terms of mass and velocity. Here, as in the case of Sensitivity, he asserts that we do not have a measure in the usual sense of the word: instead of measuring the thing itself, we measure something related to it, dependent on it, covariable with it, and thereby attain an *indirect measure.* In such wise, he says, *Kraft* is *indirectly determined* by empirical mass and velocity, while *Empfindlichkeit* is *indirectly determined* in the manner that we have just mentioned. He concedes, moreover, that both procedures are arbitrary, and that each of them yields 'merely a fact of definition,' since neither *Kraft* nor *Empfindlichkeit* is determinable except by the procedure that is specified in the defining equation.

[2] Although the word *Kraft* is now universally employed to denote Force $= M \cdot d^2s/dt^2$, the practice was not general in Fechner's day. The context of the passage cited indicates that he used it in this place in the sense of *lebendige Kraft* $= E = \frac{1}{2}MV^2$, of which he had just been speaking, despite the fact that elsewhere he observes the verbal distinction. Fortunately, the question is not important to our argument, as we are concerned with the form of his reasoning, which is not greatly affected by the distinction.

Thus, he sets a precedent for the Mental Tester, of saying that a thing is *indirectly measured* by that which measures it *directly*, and of discussing a given mode of determination as if it were *alternative*, and yet at the same time *unique*. Such discussion, of course, makes a fair grade of nonsense; it is surprising, in a way, to find it in Fechner's writings, for he is usually precise in definition and accurate in his reasoning. Still, it is there, exceptional though it may be, and it leads to important and evil consequences later in his treatment.

One suspects that Fechner's ordinary attitude is expressed in a remark concerning scientific method which was made by Kohlschütter, an intimate pupil of his when the 'Elemente der Psychophysik' was nearing completion. Though anachronistic, it savors of prophecy. "Just as physics dates its progress from the time when it ceased to speculate on Essence and Idea of events and forces, and contented itself with knowledge of their formal laws; from the time when its kinetic Energy [*lebendige Kraft*] became nothing but the half-product of the mass into the square of the velocity, and Velocity nothing but Space divided by Time: so, the season may have about come for physiology also to try its hand with strictly formal concepts" (**16**, 211).

It is therefore well to remain alert while we examine the Fechnerian concept of *Empfindungsgrösse*, and the procedure which results in its determination, lest we be misled by such ambiguities into treating facts of definition as if they had independent existence. We shall treat the fundamental equation [1] and the equations deduced from it as formal definitions, until we can settle the question whether observational data can be found which fit into the forms.

We come next to an examination of the procedure by which the Sensitivity (*Empfindlichkeit*) of the subject to a given kind of change in the stimulus-setting is determined. It is useful to regard this procedure as being essentially like a *calibration* of a laboratory instrument which is employed as a detector of changes in the physical world. We do use the reacting organism as such a detector, only, our chief interest lies in establishing the characteristics of the detector rather than in the character of the changes which affect it.

One chooses a series of stimuli, such as $R_1, R_2, \cdots, R_n, \cdots$, and compares them with respect to some particular kind of response which they evoke in varying degrees of frequency or probability. All these stimuli are of the same kind: *i.e.*, all of them are lights, or else sounds, *etc.* They must have one dimension in common, such as area, intensity, or duration; this dimension is allowed to vary in a manner which is taken into account, while all other dimensions are kept constant if possible, or at least prevented from varying systematically. Using Fechner's notation, we shall designate the experimentally varied dimension by the symbol β.

The next essential step is the selection of a set of responses which are arbitrarily taken as critical for the detection of the stimulus-changes to be considered. We must demand, at the least, one response for the specific case in which $\beta_2 > \beta_1$, and another for the case in which $\beta_2 < \beta_1$.[3] No organism could be employed which had not a repertory of at least two responses. The critical responses must be distinguishable, or at least result in records that are distinguishable, the one from the other, *to an onlooker*. The *form* of the critical response is in a sense arbitrary. If the reactor is a normal human adult, we may quickly impose a conventional habit upon him by verbal instruction. We might choose to require him to press one key in case $\beta_2 > \beta_1$, and a different key in case $\beta_2 < \beta_1$. Or we might give him only one key and require him to press it once in the first case and twice in the second. We might require him to use words, *e.g.*, *Right* or *Left*, *First* or *Second*, in answer to the question (stated or implied) at each presentation, namely *Which of these stimuli* (with respect to dimension β) *is the greater?* Or we might require the answer *Yes* or *No* to such a constant question as *Is the first of these stimuli the greater?* Or we might require him to talk or write in the technical jargon of our favorite 'school' of psychology. We need not limit him to the use of categorical answers, but if we do not, we must categorize such answers

[3] Hereafter, when we express a comparison of two stimuli (R's) or of two stimulus-magnitudes (β's), the one indicated by the smaller numerical subscript is to be taken as the standard member of the comparison, and the other member as the variable.

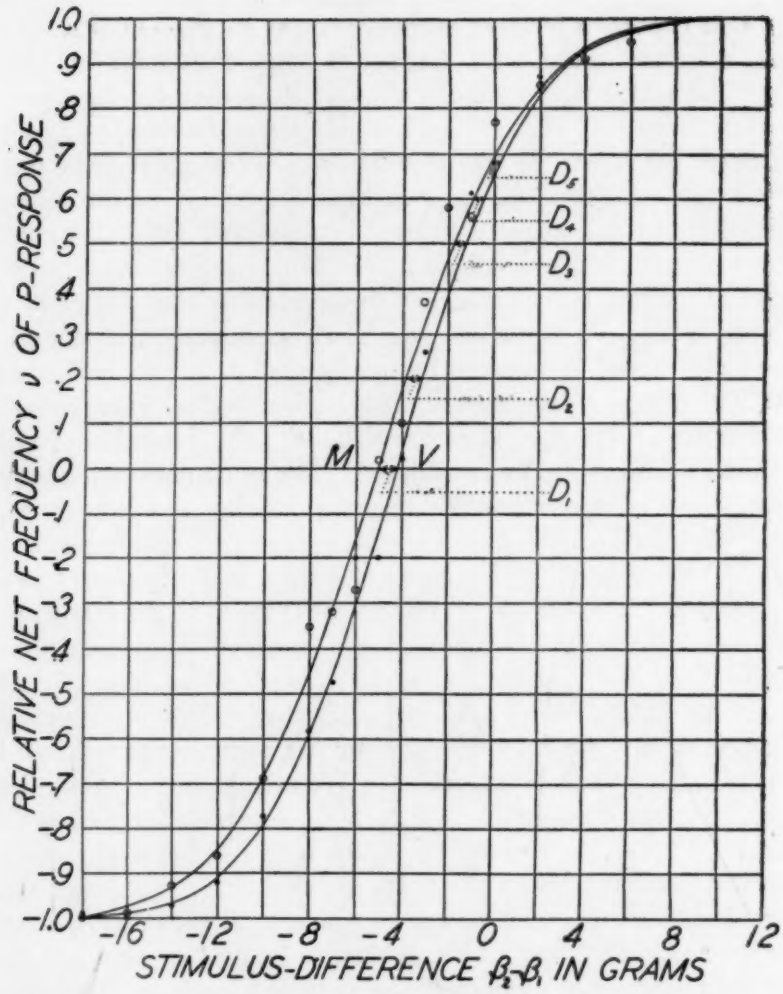

FIG. 1. Relative net frequency ν of 'positive' responses exhibited as a function of the magnitude of the stimulus-difference $\beta_2 - \beta_1$, in two sets of experiments, *V* and *M*, made on the sensitivity of one reactor to differences of lifted weights. By instruction the 'positive' response was made appropriate when the variable stimulus β_2 was heavier than the standard stimulus β_1. The variable stimulus was always presented after the standard, and on its left: hence a large and interesting constant error.

In set *V* the critical response was vocal; in set *M* manual, as described in our own text. The differential effect attributable to the mode of response is represented by D_1, D_2, D_3, D_4, or D_5, according as the standard net frequency is taken as 0, 0.2, 0.5, 0.6 or 0.68: *the 'effect' is relative to the criterion!* If $\nu_0 = 0$, the differential effect is 0.76 grams, and is ten times as large as its probable error.

The constants of the two $\varphi(\gamma)$ curves fitted to the data are as follows: for curve *F*, $X_0 = -4.3$ grams, $h = 0.157$; for curve *M*, $X_0 = -5.06$ grams, $h = 0.144$.

The experimental data are taken from the report of Warner Brown (3). Each plotted point represents the result of a census of 200 responses, there being 19 points, representing 3,800 responses, in each of the two sets.

as he may return, as in the example given by Titchener (19, 37); *but such a procedure reduces essentially to the use of a variable rather than a constant criterion.*

If the organism to be studied is an animal such as the dog, the monkey, or the domestic fowl, we may train it to lift one foot in case $\beta_2 > \beta_1$, and another foot in case $\beta_2 < \beta_1$; or to go to one feeding place in the first case and to another in the second. If the reactor is an organism such as the oat-seedling, or *Stentor*, for example, it is better not to try to train it, but rather to make use of some specific response which the organism already exhibits, like turning or traveling toward the one stimulus or the other, according to the direction of the difference in their magnitudes.

Although, as Bentley (1b) points out, the choice of a critical response is arbitrary, it is not true that it has no influence on the result of the determination of Sensitivity. The value yielded by the determination is strictly relative to the kind of response employed. For example, Warner Brown (3, 41 *ff.*), found that the sensitivity of one reactor to differences in lifted weights was about 18 per cent greater if she was required to answer *Heavy* or *Light* to the constant question *Is the second* [weight] *heavy or light* [in comparison to the first]? than if she was required to push toward the experimenter the weight which he was to record as the heavier. The difference between the two determinations, each of which was based on 3,800 observations, I find to be about ten times as large as its probable error. The relationship is shown, along with some others to be presently mentioned, in our Fig. 1.[4]

[4] I recommend this article of Brown's to those physiologists who may have been impressed by Anrep's derision (1a) of a suggestion of mine, tendered sixteen years ago to the effect that a frequency-difference between two stimulus-tones might suffice to make a dog salivate, while it might not suffice to make him go to one feeding-place rather than to another. The suggestion then seemed to be a tenable hypothesis for explaining the positive results reported by certain of Pavloff's students on tone-discrimination in the dog, and the negative results which I had obtained by the use

Here—and I suspect the principle is universal in scientific experimentation—the magnitude of the effect which the experiment will exhibit is determined *ceteris paribus* by the criterion. The determination is meaningless unless the criterion is kept constant and unless it is specified.[5]

If the required response occurs at any stimulus-presentation when the stimulus magnitude $\beta_2 > \beta_1$, for instance, then we say that the stimulus-difference $\beta_2 - \beta_1$ was *effective;* if the response demanded does not occur we say that the stimulus-difference was *ineffective.* We classify each presented stimulus-difference categorically: either it was followed by the required response or else it was not. All *gradations* of effectiveness, as we shall presently see, are statistical conventions, established by definition.

If we now turn to the fundamental methods of psychophysics, we shall find that in every case, the experimental procedure which results in a determination of Sensitivity consists in *classifying stimulus-differences* (such as $\beta_2 - \beta_1$) according to two independent criteria taken simultaneously. One asks (*a*) What was the magnitude of the stimulus-difference, and (*b*) Did it evoke the prescribed response or not? These questions are answered by the experimenter, not by the reactor, and any significance which their answers may have to the reactor's examination of his mental content is placed upon them by assumption.

Consider first the so-called Method of Just Noticeable Differences, taking it first in one of its crudest forms. (To make the situation concrete to the cultural reader, let us suppose that the stimuli R_1 and R_2 are two luminous surfaces whose brightnesses β_1 and β_2 are to be compared, β_1 being

of a differential locomotor reaction. While the principle is not to be dismissed as lightly as Anrep attempted to dispose of it, I now believe that the facts of the two investigations are more simply accounted for. Pavloff's recent book (17) has made it clear that his school employed, not a constant criterion, but a variable one. It is extremely doubtful whether the differential salivary response was determined by the tones at all.

[5] This principle can be applied to great advantage in many other cases which now claim the attention of the scientific public: *e.g.*, *quantum*-emission, the doctrine *all-or-nothing* of the response of living tissue, and the so-called *curve of learning.* Much discussion which is now meaningless could thus be clarified.

kept constant and β_2 varied.) Let us employ the symbol P to designate the response demanded when $\beta_2 > \beta_1$ and the symbol Q to denote the response required when $\beta_2 < \beta_1$. The experimenter may begin by setting the lamp which produces β_2 at such a position that $\beta_2 \approx \beta_1$, and then allowing it to be moved, through small distances at a time, until the reactor makes the response P. This position is recorded for future computation of the stimulus-difference $\beta_2 - \beta_1$. The experimenter now classifies all stimulus-differences less than this value as *ineffective* (in the production of the response P at the time this particular comparison was made); and this value and all which exceed it as *effective* in the same respect. To avoid confusion, however, he must be careful in the naming of what he has discovered. He has not observed that if this value of $\beta_2 - \beta_1$ were diminished by a given amount, the response P would fail to occur; nor yet that if the stimulus difference were increased by a given amount the response P would be given. The designation *least effective difference presented* is therefore much better than *just effective difference* or *barely effective difference*, for either of these designations would suggest rather more than has been observed.

The experimenter then causes the variable lamp to be moved still nearer to the screen, so as to increase β_2 still farther, and then allows it to be moved away by small steps until the reactor ceases to give the response P. He now determines the new value of $\beta_2 - \beta_1$, and classifies it and all smaller differences as *ineffective*, and all larger differences as *effective*. An appropriate designation for this difference is the *greatest ineffective difference presented* [at the time the comparison was made].

If it is demanded, at what point is the stimulus-difference *just effective*, or *barely effective*, one must reply that the empirical data do not yield a valid answer. To be sure, if one chooses to follow a certain fashion, one may *define* bare effectiveness as being represented by some sort of mean of the two stimulus-differences which were designated as *least effective* and *greatest ineffective*, but such a fact of definition is not empirical.

The determination of the least effective and the greatest ineffective differences with respect to response *Q* follows the same general plan as the one we have just described.

I pause here to note that in every case in which we have thus far said, '*the stimulus-difference was effective*,' the more conventionally trained experimenter would have said, '*the* [corresponding] *sensation-difference was noticeable*,' or '*noticed*,' or '*perceptible*' or '*perceived*.' Is our set of verbal forms equivalent to his? Does either of them indicate to another person anything except that '*the prescribed response occurred*'? While these questions are not purely rhetorical, we shall postpone their consideration to another place, and return to the experimental procedure.

Either of these groups of stimulations has resulted in a classification of a whole series of stimulus-differences as *effective* and *ineffective* in the production of (*e.g.*) response *P*; but the classification is not ideal. To begin with, we have classified stimulus-differences which were not presented to the reactor: namely, those which lie outside the range of the experiment. We have pretended that stimulus-differences which lie beyond the two critical points would not have produced a response of the opposite class if they had been presented—a proposition which is not supported by experience. Says Fechner (**8**, i, 74): "Neither the first point at which a sensational difference becomes just noticeable, nor yet [the point] at which it vanishes, can be determined very exactly; one passes through an interval of uncertainty whether it is noticeable or not. If one does not wish to set the degree of bare noticeability [*Grad des Ebenmerklichseins*] somewhat too high—*i.e.*, to consider as barely noticeable only such a difference as will *certainly* and *unexceptionally* appear noticeable upon repetition of the experiment,[6] notwithstanding that a somewhat smaller difference also must often appear noticeable: then the method is automatically converted into that of

[6] *Query*: Would such a treatment render the notion of *bare* noticeability meaningless? One suspects that this is one instance in which *ebenmerklich* is used in the sense of *equally noticeable* rather than *just noticeable* or *barely noticeable*, although Titchener and other able expositors appear to take the other view. It is well to recall, however, that the expression was not then strictly technical.

Right and Wrong Cases, for instances always occur in which one is deceived or in doubt as to the direction of the difference: cases which are to be considered in proportion to their frequency." (The italics are Fechner's.)

The refined form which this method eventually assumed is not Fechner's Method of Right and Wrong Cases, but one which is somewhat more general than the latter, and which, in a sense, includes the latter as a special example. It goes by various names, one of which is the *Method of Constant Stimulus-Differences*. An arithmetical series of values is chosen for β_2, each of which is presented with β_1 a considerable number of times, according to the degree of precision which is demanded. The order of presentation of the members of this series may be either haphazard or regular, according as certain constant sources of error are to be eliminated or studied. The reactor is required to give one of two responses, P and Q, according as $\beta_2 - \beta_1$ is positive or negative. A rule must be applied to doubtful answers. Some investigators forbid them, and require the reactor, in case of doubt, to guess. Warner Brown (**3**, 28), declares:

> The direct method, . . . *by finding the frequency with which each type of judgment occurs with any amount of* [stimulus-] *difference*, aims to discover whether or not there is any . . . point where judgments of a particular kind begin or leave off. This aim is defeated unless the kinds of judgment employed are mutually exclusive. Evasive answers . . . permit the observer to avoid forming a judgment in just those cases where it is most important to discover whether or not a correct judgment can be formed. Any method of counting such answers begs the whole question by assuming to know what the observer himself would not tell, namely, how strong an inclination he had to judge one way or the other, that is, how great a probability there was that a certain judgment would have been made if it had been necessary to make one.

On the contrary it is sometimes urged that the reactor should not be constrained by instruction to give a response which the experimental stimulus-difference would not otherwise evoke. It may be replied that if the reactor is a human, or a trained animal, and the response a conventional one, it is

due to the constraint imposed by instruction or other training, or by the setting of the experiment (*cf.* the Yerkes box), that the stimulus-difference, however large it may be, evokes the required response at all.

We need not dwell further on the disputed rule, except to emphasize that equivocal answers are foreign to the method, which makes no provision for their treatment as such. The experimenter may forbid them by instruction, or by arranging the experimental situation so that one of the responses P or Q *must be given* (*cf.* again the Yerkes box); he may permit them to be given and exclude them from his census; he may permit them to be given and treat them according to the rules of the Method of Average Error; or he may pretend, as Fechner did, that they were not really equivocal, but that a certain proportion, such as half, belonged to class P and the remainder to class Q. But this method provides only for the use of categorical responses.

The effectiveness of the stimulus in the production of the response P is defined as some function of $[P]$, the number of responses of class P, and N, the number of responses in both classes, or $[P] + [Q]$. The function is chosen on grounds which are partially arbitrary, but Jastrow (**12**) presents a good argument for the choice of one which we may designate by the symbol ν. If nothing but chance factors were operative, we should expect, 'in the long run,' to obtain $[P] = pN$, and $[Q] = qN$, in which p and q denote the chance-frequencies of responses belonging to class P and class Q respectively, and $N = [P] + [Q]$. The number $[P]$ of class P responses obtained by actual count is therefore diminished by the number $p[Q]/q$ which, on this assumption, represents the number of class P responses attributable to chance. But inasmuch as under the now conventional plan of experimentation, $p = q = \frac{1}{2}$, and $p/q = 1$, the conventional though special definition of ν would be as follows:

$$\nu \equiv \frac{[P] - [Q]}{[P] + [Q]} = \frac{[P] - [Q]}{N} = \frac{2[P]}{N} - 1, \qquad [4]$$

so that ν represents (on the assumptions mentioned) the

relative frequency of P-responses not attributable to chance. Fechner's fundamental table could be based upon v as the argument by substituting $(2r/n) - 1$ for r/n in the first column.[7]

We may pause here to note that the procedure we have outlined is characteristic of all the other representative psychophysical methods, to this extent: all of them proceed by measuring the stimulus-differences with which the subject is confronted, classifying them categorically, according to a conventional, or social, criterion, namely according as they were followed by a prescribed response or not, and then counting the responses belonging to each class. The differences are non-essential: in the Method of Right and Wrong Cases, the class-designations are *right* and *wrong* instead of *positive* and *negative*, or P and Q. In the Method of Average Error, the categories are *equal* and *unequal*, and the census shows how frequently each magnitude of stimulus-difference evokes the response *equal*. Since the procedure is universal in so far as its essentials are concerned, we shall return to the Method of Constant Stimulus-Differences for most of our illustrative details.

We have defined the *effectiveness* of a stimulus-difference as identical with the net frequency v of a selected class of response, making the determination relative to the response we chose as critical. The next step in the determination of Fechnerian Sensitivity is the adoption of some *standard* of effectiveness, which we may designate by the symbol v_0. We are now to find the magnitude of that stimulus-difference $\beta_2' - \beta_1$ which yields a net frequency v_0 of class-P responses; for its reciprocal $1/\beta_2' - \beta_1$ is to define the Sensitivity of the reactor to the stimuli in question, under the restricting conditions of the experiment. The question is, what value shall we select for v_0?

The choice is arbitrary, and varies from experimenter to experimenter. We might ordinarily content ourselves with

[7] This reasoning is a generalization of Jastrow's, and the notation is our own. Although a perfectly valid experimental situation can be arranged, and in certain special circumstances demanded, in which $p \neq q$, I shall defer its discussion to another paper, since it has not yet actually come into frequent use.

saying that any convenient value might be selected, provided only that it be kept constant in the various intercomparisons, and that it be specified. However, different experimenters, who have used different standards of effectiveness, have put the same psychophysical interpretation on all of them. We may therefore do well to look into the matter.

If we plot algebraic values of v against algebraic values of $\beta_2 - \beta_1$, the points often lie on or near a portion of an inflected curve, which sometimes looks like the curve that represents the probability-integral. It has long been fashionable to assume that the latter curve is the proper one to be fitted to the data, and the existence of convenient tables for computation of its constants from empirical data has often led to its use when the asymmetry or the complexity of the distribution invalidated such a form of description. Now the inflexion-point of this curve represents $v = 0$. As the curve is nearly straight for some little distance on either side of the inflexion-point, it is possible to locate the latter pretty accurately by means of graphic interpolation. On *this* curve, the inflexion-point represents that value of $\beta_2 - \beta_1$ a given change of which results in a maximum change in v.[8] Some investigators therefore take $v_0 \equiv 0$.

Jastrow (**12**), (by implication), defines $v_0 \equiv 0.5$, for the reason that on the probability-integral curve it represents $hD = 0.5$ and $D = E_D$, its own probable error. According to Titchener (**19**, 283), Fechner preferred to take $v_0 = 0.68$ (*i.e.*, $r/n = 0.84$), since it represents $hD = 1/\sqrt{2}$, and $D = \sigma_D$, its own standard deviation. For the Method of Serial Groups, Stratton (**18**) by implication defined $v_0 \equiv 0.6$, on grounds which he designated as arbitrary. Other investigators have used $v_0 \equiv 0.2$, on similar grounds.

[8] In actual practice, curves are often obtained which have more than one inflexion-point, and in which the steepest slope corresponds to some value of v other than 0. Warner Brown (**4**, 242 *ff.*) proposes the use of the value of $\beta_2 - \beta_1$ which corresponds to the maximum change in v for use as the standard of comparison, regardless of the absolute value of v which it represents. His argument for the appropriateness of such a value is rather impressive, but his procedure in cases in which the curve has more than one inflexion-point, with about the same slope at each, might be worked out further. The mean of their abscissal distances appears to represent nothing of peculiar importance.

Certainly, if the critical value of ν is constant, all the stimulus-differences which belong to it are by definition *equally* effective, whatever the absolute value may be. It does not follow, however, that they are consequently *just* effective, or *barely* effective. Only *one* value of ν can possibly have this defining property. This rather obvious point will require mention a little later.

It is now possible to state the empirical facts which are asserted in Weber's law in a form which is independent of the kind of response that may have been chosen as critical, and independent of any assumption concerning the relationship of ν to the change in the intensity of introspectional sensation. The law is satisfied in case

$$\frac{\beta_2 - \beta_1}{\beta_1} = *** = \frac{\beta_3 - \beta_2}{\beta_2} = *** = \frac{\beta_n - \beta_{n-1}}{\beta_{n-1}} = \kappa$$

and in case also

$$\nu_1 = *** = \nu_2 \qquad = *** = \nu_{n-1} \qquad = \nu_0,$$

in which the several ν's bear the same subscripts as the standard stimulus in the comparisons which produced them. Expressed verbally the statement of the law would read: *In case the stimulus-difference bears a constant ratio κ to the magnitude of the standard stimulus, the net frequency of responses of class P is a constant, ν_0, irrespective of the absolute magnitude of the stimulus-difference.* Whether the law is universal, and within what ranges of absolute stimulus-magnitudes it holds with acceptable precision, are important questions, but do not affect our present discussion, which is concerned chiefly with the interpretation of the law as a formal statement.

Thus far, we have followed good psychophysical procedure, and have departed from convention in only two respects. First, we did not require our reactor to differentiate the compared stimuli by talking or writing about sensations. In fact, we chose not to limit our choice of reactors to the language-using class, being interested, upon occasion, in studying other organisms than human. Secondly, we have postponed until now all discussion of the relevancy to introspectional sensation, or to Fechnerian sensation, of the

strictly empirical facts which, within definite limits and subject to restrictive conditions, are described with tolerable precision by Weber's law. The postponement was made because the facts which are yielded by physical measurement of stimuli and the classification and counting of responses have no intrinsic reference to sensation, and the sensational meaning which they are sometimes made to bear is imposed upon them by assumption.

This imposition was accomplished by Fechner, in a very illusive manner. Although its assumptional character has been affirmed, *e.g.*, by Warner Brown (**3**, 18), and by Watson (**20**, ch. i), the writer has never seen it exhibited, and it seems usually to have escaped detection. It begins with Fechner's interpretative statement of Weber's law, which may be put thus:

If

$$\frac{\beta_2 - \beta_1}{\beta_1} = *** = \frac{\beta_3 - \beta_2}{\beta_2} = *** = \frac{\beta_n - \beta_{n-1}}{\beta_{n-1}} = \kappa,$$

then,

$$\gamma_2 - \gamma_1 = *** = \gamma_3 - \gamma_2 = *** = \gamma_n - \gamma_{n-1} = \Delta\gamma = \text{const.}$$

in which each particular γ denotes the intensity of the Fechnerian sensation that corresponds to the stimulus-magnitude β indicated by the same subscript. *If the stimulus-difference bears a constant ratio κ to the magnitude of the standard stimulus, it results in a constant increment $\Delta\gamma$ to the magnitude of the corresponding* [Fechnerian] *sensation.* If κ is made sufficiently small, $\Delta\gamma$ approaches zero as a limit, and may be indicated by the symbol $d\gamma$ and treated as infinitesimal.

Note that our own statement of Weber's law is based strictly upon what the experimenter *does*, according to the method selected for illustration. Fechner's statement substitutes an *interpretation* for the second proposition of our own. The two statements are equivalent if their second propositions are equivalent: *i.e.*, if a stimulus-difference which arouses a standard net frequency v_0 of positive responses *necessarily* produces a constant increment to the magnitude

of sensation. Fechner, indeed, asserts this equivalence: in (**8**, i, ch. vi), he defines Sensitivity as the reciprocal of the stimulus-difference which produces a constant increment to the intensity of sensation; while in ch. viii, he defines it (according to the Method of Right and Wrong Cases) as the reciprocal of the stimulus-difference which produces a constant proportionality of 'right' answers, which is only another way of saying a constant net frequency ν_0 of such answers, because ν is only a conventional restatement of r/n.

Thus, $\Delta\gamma$ and ν_0 are mutually interchangeable in his defining equation and therefore mathematically equivalent. His statement of Weber's law is certainly not strictly empirical, despite the fact that he called it so, unless the expression 'constant increment of sensation' means purely and simply a constant preponderance of responses in a particular conventional class. In such case he has accomplished a change of name: nothing more.

His fundamental formula [1] assumes this equivalence, and further assumes that the properties of small but finite differences persist to the limit zero. The latter assumption may be questioned, but to exactly the same degree as in physics. His formula of measurement, usually called the *Massformel*

$$\gamma = k \cdot \mathrm{Log}\ (\beta/b) \qquad [5]$$

is obtained from [1] by integration, and rests upon the same assumptions, although the formal procedure is valid. It defines certain essential properties of Fechnerian sensations, which are presently to be compared with the properties which are affirmed of introspectional sensations, and therefore requires a little examination.

If the *Massformel* [5] is valid,

$$\begin{aligned} \gamma_1 &= k \cdot \mathrm{Log}\ (\beta_1/b), \\ \gamma_2 &= k \cdot \mathrm{Log}\ (\beta_2/b), \\ \gamma_2 - \gamma_1 &= k \cdot \mathrm{Log}\ (\beta_2/\beta_1). \end{aligned} \qquad [6]$$

As β_2 approaches β_1 as a limit, β_2/β_1 approaches unity, its logarithm approaches zero and $\gamma_2 - \gamma_1$ also approaches zero

as a limit. This condition is said to be fulfilled when the net frequency of positive responses r approaches the arbitrarily selected standard r_0; when $\beta_2 - \beta_1$ produces r_0 frequency of *P*-responses, the corresponding increment to sensation $\gamma_2 - \gamma_1 = d\gamma$ is said to be infinitesimal and ≈ 0. *Liminal differences in magnitudes of Fechnerian sensations are very nearly equal to zero, and therefore to each other.* Is this property shared by introspectional sensations?

I do not claim to be an introspector, and I am not sure that I have ever owned an introspectional sensation. My acquaintance with the properties of such things may be uncertain: I assume for the purposes of discussion that those properties are what introspectors tell me they are. The example I am about to give may not be suitable, but I shall present it for what it is worth. I have some considerable skill, considering my limited experience, in photometry. When the movable lamp occupies a certain position, the pattern on the photometric field is visible: every time the field is exposed I see the pattern. As I move the lamp toward the position of balance, I do not always see the pattern: now I can distinguish it, and again I cannot. But when it is visible, I cannot tell from simple observation *how* visible it is. That is to say, there is no point at which I could say that if it were made the least bit fainter I could no longer distinguish it, *except by inference.* That inference is based on my estimate of the number of times I distinguish the pattern as a fraction of the number of times I examine the field, or on my estimate of the fraction of the exposure-time during which I can make the distinction. In other words, my judgment of *bare* visibility is based on the same assumptions and on essentially the same procedure as the experimenter employs in the Method of Constant Stimulus-Differences or the Method of Right and Wrong Cases, only it is cruder. If this be introspection, well and good: even a behaviorist can introspect. But my account of the matter differs from Boring's, for he says that such differences can be perceived as *just* noticeable.[9]

[9] While I shall not now take the time to consider the question in detail, I do not consider that certain of Bentley's scruples about referring to the 'pattern,' the 'field,'

Thus, within narrow limits, at the least, he makes the degree of noticeability of a sensational difference determinable by direct comparison of the sensations, using introspectional indifference as a guarantee of equality of their magnitudes. But introspectional indifference does not *guarantee the equality of the kind of sensations whose magnitudes are defined by the Fechnerian Massformel!* This important property is not common to the two kinds of sensation, and the fact precludes their identification. Let us examine the evidence.

As Fechner himself pointed out (**8**, ii), if the *Massformel* is true, it follows that

$$If\ \beta > b, \qquad \gamma > 0. \qquad [7]$$

$$If\ \beta = b, \qquad \gamma = 0. \qquad [8]$$

$$If\ \beta < b, \qquad \gamma < 0. \qquad [9]$$

$$If\ \beta = 0, \qquad \gamma = -\infty. \qquad [10]$$

It is to be kept in mind that b represents that value of the stimulus-magnitude β which yields the standard net-frequency ν_0 of P-responses; and furthermore, that if enough readings are taken, it will be found that increases of β above the critical value b yield a value of $\nu > \nu_0$. This is implied in proposition [7] since it is asserted that $\gamma > 0$ if $\nu > \nu_0$.

Proposition [8] affirms that if the stimulus-magnitude β equals the value b which produces the standard net frequency ν_0 of P-responses, the magnitude of the corresponding Fechnerian sensation is equal to 'zero.' That this proposition states a mere fact of definition appears from the fact that it is affirmed, by one psychophysicist or another, whether ν_0 is defined as 0, 0.2, 0.5, 0.6, 0.68, or whatever other value it may please him to select, whereas it is empirically impossible

and the 'brightness' are justified. The pattern and the field can be completely specified in terms of spatial distributions of brightnesses; and brightness itself is a valid objective concept. *Cf.* my chapter in Watson (**20**). Brightness has been called a strictly physical concept, although one of its factors, called the 'visibility constant' is partly social, depending in part on a classification and census of the responses of the reactor in the calibrating experiment. I suspect that this much of the difference between the two modes of description will be found specious, reducing to a choice of terms, made on arbitrary or temperamental grounds.

that more than one of these values should have this peculiar significance.

Having defined the 'zero-point' on the scale of intensity of Fechnerian sensation by some arbitrarily chosen value of ν_0, the psychophysicist affirms that at the stimulus-threshold, all intensities of Fechnerian sensation are equal to each other, since all of them are equal to 'zero.' So they would be, if they were all equally distant from an absolute null-point: *i.e.*, from one which represents an utter absence of sensation, in the sense in which $-273°$ C. is conceived as representing a complete absence of molecular motion. Are they equal in such a sense? Whether the question is answerable or not depends on the location of this absolute null. If it cannot be located, the question states a pseudoproblem.

As Fechner directly asserts, and as Kohlschütter (**16**,211), expounds the question, the 'zero' of Fechnerian sensation defined in [8] is not absolute null, but a mere arbitrary reference-point. The use of positive and negative signs indicates nothing but opposition: the magnitudes are measured in different directions from this point. Proposition [9], which is as valid as the *Massformel*, affirms that if the stimulus-magnitude β is finite, but less than the value b which produces ν_0 P-responses, the magnitude γ of the corresponding Fechnerian sensation, though less than the arbitrarily chosen value designated as 'zero,' is finite: although the owner of the sensation is unconscious of its presence, incapable of observing its properties, and 'asleep' to the stimulus which produced it. This doctrine, in fact, constitutes the formal basis of Kohlschütter's attempt to study Fechnerian sleep. It has caused some trouble to the neo-Fechnerians, who would prefer to explain it away, but it is as valid as his defining equations and the operational laws of mathematics. The 'unsensed sensation' is indeed as important to the Fechnerian system as the other kind: it is through the concept of 'uncognized consciousness' that he finds the continuity which he postulates to exist among the consciousnesses of different organisms, and also among those of one organism at different times. Since the organism at every instant is assailed by in-

numerable stimuli, most of which produce no response of the kind that the psychophysicist accepts as critical (though they may serve to intensify or to impair responses that are deemed specific to other stimuli), it follows that at every instant the organism is 'awake,' in Fechnerian terms, to only a small part of its environment, and 'asleep' to all the rest. This condition of partial or distributed sleep is Fechnerian 'Attention': a concept which I have found useful, and which is capable of representation in objective terms and assimilated to the postulates of behaviorism. But it means this: that at every instant the organism has innumerable Fechnerian sensations of which it cannot be aware, amongst which huge differences in magnitude may exist, although they are introspectionally equivalent.

It remains to examine the notion of the sensational difference-threshold. Two stimulus-magnitudes, such as β_2 and β_1 are employed, such that the net frequency ν_0 of class P responses is obtained. It is then affirmed that the corresponding sensational intensities γ_2 and γ_1 are about equal. Suppose for the moment that γ_2 and γ_1 represent intensities of introspectional sensations: what does the judgment of 'equality' mean? It should mean that their absolute magnitudes are about equal; that they are about equally distant from an *absolute* null. But from proposition [10] it appears that the absolute zero of the Fechnerian sensation lies below the stimulus-threshold—more specifically, at minus infinity. If Fechnerian sensations and introspectional sensations are identical, then the proof that two introspectively equivalent sensations are equal in magnitude becomes impossible. The critical operation requires the direct observation and comparison of subliminal sensations, which have the unfortunate property of being unobservable. If introspective indifference is taken as a guarantee of equality, then the identification of introspectional sensations and Fechnerian sensations must be abandoned, and the fundamental formula, with all of its consequences, must be treated as formal definitions. They prescribe a means of determining the magnitude of something which is *called* sensation, but which lacks the essential properties thereof.

I yield to none in according respect to Fechner for conceiving an important problem, and subjecting it to extraordinarily detailed and painstaking analysis. It is impossible that so much thought and study could be invested without resulting in a valuable by-product. That he should have failed, as he did, to identify the critical operation, and discover its impossibility, is probably explanable on grounds which Bentley would call 'biographical.' He was seeking a basis for a thoroughgoing Panpsychism: for talking as if all Movement denoted Life; and as if all moving objects, being 'alive,' were endowed with Soul. His reasoning is usually rigid: he assumed his conclusions in his definition of Life and Mind in terms of physical Motion. Fechner, be it remembered, was an able physicist and mathematician and well versed in the Theory of Measurement. Can one imagine Fechner the physicist resorting to extrapolation to determine a function far beyond the limits of observation? Yet his *Massformel*, if applied to introspectional sensation, pretends to establish a relationship, which at best could be claimed to hold only through a restricted range, as holding between the terminal threshold at one extreme and minus infinity at the other. His zealous concern for a philosophic system led him to abandon scientific precaution in the attempt to establish it.

Panpsychism, to be sure, would be a respectable system if it were completely worked out. If one insists on saying that a reactor was *conscious of* a change instead of saying that he *reacted specifically to* the change, there can be no possible objection, except on grounds of taste. But it is proper to request that any expression be used according to a uniform rule of discourse. If specific response is to define consciousness on the part of the reactor, then let us attribute consciousness to everything that reacts specifically. Let us affirm it, not only of human adults, but also of blood-corpuscles, of oat-seedlings, of voltmeters, of selectively permeable membranes, of stars and of electrons, just as Fechner would have done. Only, when we shall have perfected our panpsychistic vocabulary and put it into use we shall find that we have merely adopted *another conventional*

fashion of describing the world of physical events. That fashion will be less concise and less simple than one which is based on the concepts of universal science, but it will afford more æsthetic and emotional gratification to those who prefer it. The question seems to be, whether the net gain promises to be sufficient to justify the effort of learning another language. Viewed in this light, most of the arguments for and against a Panpsychism seem to be attenuated.

Dr. Boring's inquiry, "What in the world is β, the 'intensity' of a stimulus," is a little surprising, if he means to inquire how Fechner defined it. In (**8**, i, ch. iv), he defines it in terms of kinetic energy (*lebendige Kraft*), or rather in the equivalent terms of Work performed on the receptive apparatus of the organism. Some obscurity arises later through the introduction of an assumption, sometimes tacit, sometimes overt, that the energy which is transformed within the organism is directly proportional to the energy contributed by the stimulus: an assumption which was highly important to his argument, but which has been thoroughly disproved.

I suspect, however, that Dr. Boring meant to ask something different from this, and I agree with him that the term 'stimulus-intensity' has been abused. It connotes, of itself, a *distribution* of work, either in time, in space, or in both. An examination of some of the best experimental studies which bear on Weber's law might yield a far more valuable return than more experimentation at the present time. For example, in the case of sensitivity to cutaneous pressures, the critical property of the stimulus appears to be, not Work, but Power, in case the area to which the pressure is applied is kept constant; or, if the rate of application is held constant, and the area to which it is applied allowed to vary, then the critical property of the stimulus would seem to be work per unit area. In terms of Fechner's original definition, Weber's law probably is not satisfied in the field of cutaneous pressures.

The case is even more definite in the field of brightness-discrimination. Koenig's classical experiment (**15**) has been cited—by myself (**13**), amongst others—as proving that

Weber's law is satisfied in the judgment of brightness-differences between the limits of 1 and 200 millilamberts, according to one standard of precision, and between the limits of 1 and 1,000 millilamberts according to a more lenient standard. In this experiment, however, the reactor was allowed an unlimited time of exposure of the stimulus, which was always adequate: hence, the critical property of the stimulus was Power: the temporal rate at which Work was done upon the retina. The ten years of experimentation of Cobb and his co-workers which followed upon his acute analysis (5) of the stimulus-situation in visual observation, have shown that in respect to differential sensitivity to brightness, Weber's law, in terms of Fechner's definition of the stimulus-variable, is not even a tolerable first approximation to a true description of the facts. For, in these experiments, the time of exposure was restricted so that the critical property of the stimulus became not Power but Work, thereby satisfying Fechner's definition of stimulus-intensity. The significance of these findings to problems of illumination is very great, although the illuminating engineers appear to have been slow in comprehending it.

In general, it would appear that a careful analysis of the experiments which seem to support Weber's law is demanded. It may show that if a single definition of stimulus-intensity or stimulus-magnitude is employed, we shall find a variety of relationships between the magnitude and the effectiveness of stimuli, even in those fields in which the law has usually been considered valid.

Summary

1. Fechner's fundamental formula states a mere fact of definition. His formula of measurement and its consequences, being nothing but deductions from the fundamental formula, must be treated as mere formal expressions, and not as descriptions of introspectively observable facts, until it can be independently shown that such facts agree with the formulas.

2. The so-called *just noticeable difference in sensation*, which is the thing defined by the fundamental formula, is not

determined by direct introspective comparison, but by a double classification of stimuli and a census of classified responses. The sensational meaning of that census is imposed on it by assumption.

3. Introspectional indifference does not guarantee the equality of magnitude of the so-called sensations whose magnitudes are defined by the Fechnerian formulas of measurement; for Fechnerian sensations of absolutely large and finite magnitudes exist at every instant, without their owner being conscious of their differences or even of their existence.

4. The identification of introspectional sensation and Fechnerian sensation, though usually taken for granted, is illicit.

5. The contention of the behaviorist that psychophysics is not a genuine science or a possible empirical science is not impaired by Fechner's treatment.

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FOUR SOURCES OF CONFUSION IN PSYCHOLOGICAL THEORIZING

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Although it seems customary in theoretical articles in psychology to-day to expound some position in the light of which the author claims that all the major problems are either dissolved or no longer asked, and all the conflicting theories are reconciled, the present paper attempts to follow a different tradition. Instead of offering a new panacea, this paper attempts to analyse certain recurrent problems in psychology, and on the basis of this analysis, to point out the grounds for the differences between certain schools and individuals.

The problems to be discussed include (I) the relation between sensory processes and the stimulus, (II) the distinction between subjective and objective ('introspection'), (III) the various treatments of the mind-body problem, and (IV) the use of the methods of mathematics in the natural sciences.

I. *The problem of the stimulus.* This problem is closely linked with the treatment of the perceptual situation. Let us make a tentative analysis of the situation represented by the phrase 'I see a red book.' For the present discussion the following distinctions at least should be made.

A. There is an observer. This observer may be considered as essentially

(1) sense organs, nervous system, and muscles and glands, or

(2) awareness, in addition to these organs.

B. There is the process of observation.

C. There is the datum which is sensed. We do not see the bottom of the book, the inside cover, nor do we feel its weight and solidarity, but only a limited perspective.

D. There is the object perceived which has, actually or

potentially, all of these characteristics (Broad's 'epistemological object').[1]

E. Finally there is possibly a group of entities distinct from and causally related to the perceived object. These may be atomic or electronic in nature (Broad's 'ontological object').[2] It will be convenient to refer to these various distinctions by their letter designations.

There appear to be two classes of philosophical views on this problem held by psychologists. The philosophic question concerns the possibility of our dispensing with an additional ontological object (*E*) and accepting the perceptual object (*D*) itself as ultimate. The philosophic question asks whether what is actually sensed (*C*) inheres in and is a constituent of the actual physical object, thus identifying (*D*) and (*E*), or whether what is actually sensed is a product or consequent of the relation between the percipient and the ontological object which becomes objectified in the course of experience. We shall consider the second view first.

The traditional and more conservative psychologists usually take the accounts of physics seriously, and maintain that the molecular, or electronic theory says something about an actual stuff of the world and its relations. They would presumably assert in their psychologies that various forms of energy (various wave motions of atmosphere, and 'ether,' certain molecular forms in gaseous or liquid state, mass in acceleration, etc.), will, under the proper conditions, arouse sense organs to their characteristic modes of response.[3] The characteristic modes of response of sense organs are often called sensations. The term 'stimulus' is here justly applied to the inferred physical substrata (*E*). Although the sense-organ processes are thus in part functions of the stimulus, these forms of energy are by their very nature imperceptable. No extension, for example, in the size of our microscopes would ever transmit to our eye anything but reflected 'ether' waves,

[1] Broad, C. D., 'The Mind and its Place in Nature,' Ch. IV, (Harcourt Brace & Co., 1925) makes these distinctions and some others.

[2] *Op. cit.*, pp. 142, 150.

[3] See, for example, E. B. Titchener, 'A Textbook of Psychology,' p. 8. M. Prince gives an account in his first Powell Lecture, sec. IV and V, 'Psychologies of 1925.'

and not even these reach the intact optic nerve. The ontological entities are the physicists' best guess as to the external causal antecedents of a perception, and they are taken literally as existents. 'I see a red book' is then only a way of speaking, for actually, (on this theory) even that portion of the external surface which seems to be observed is delusive: there are merely light-waves of a certain length (not color) which are affecting the retina. The causal view of perception then assumes the existence of non-perceptual entities which cause perceptions, and can be used to explain their relations and sequences. Sensations (*C*) and perceptions (*D*) being dependent upon the sense-organs and nervous system, have all the qualitative variety which the substrata (*E*) lack; and the distinction has been emphasized by calling the one mental and the other material. The perceptual object seems to be here a product of the effects of certain stimulation on sense organs which through repetition and modification, through acquisition of meaning, or conditioning, get projected out into some kind of space.[4]

On the other hand there is a class of more recently developed views which place less assurance on the literalness of the older theories of physics, often for the reason that the physicists themselves are in the 'ego-centric predicament.'[5] The physicist may be wrong in his guess as to the actual nature of his environment; but the *facts* that he observes serve both as the initiation and the verification of his theories. The psychologist can likewise start with the facts of observation, taking them at their face value. Nor is this entirely out of line with physics itself, for even in that science appear discontented individuals who want to get back to a common-sense point of view and who hold that the older physics, in ignoring the problems of the qualitative variety of the world by the simple device of calling it 'mental,' has narrowed its

[4] Representative philosophical accounts of the causal view may be found in, 'Essays in Critical Realism' by various authors; Macmillan, 1920; Broad, C. D., 'Scientific Thought,' Harcourt, Brace & Co., 1922; Russell, B., 'The Analysis of Matter,' Harcourt, 1927.

[5] R. B. Perry describes it in 'The New Realism,' Macmillan, 1912, and again in 'Modern Tendencies in Philosophy,' Longmans Green, N. Y., 1912, pp. 129 *ff*.

field and only postponed the time when it shall have to consider colors, odors and sounds as part of nature.[6] For the new realists nothing lies behind the perceptual object (*D*): and the object is a collocation of a number of characteristics, the sum total of the actual and possible perspectives.[7] Another similar view is that of the positivists, who, with no wish to rewrite physics, regard the laws and formulas as in no sense descriptive of an actual underlying situation, but rather as a convenient way (conceptual shorthand) of expressing certain relations which hold between perceptions.[8] In perception, according to these views, we in some manner come in contact with the real object itself; the epistemological object (*D*) is the actual object. It (*D*) requires no further material entities (*E*), and the sensory qualities (*C*) actually inhere in the perceptual object, and are simply revealed to us as by a 'searchlight,' when our sense-organs are properly oriented. Illusions, hallucinations, errors, perspectives, etc., are all to be accounted for in terms of the distinction between (*C*) and (*D*). There is no good use for the term 'stimulus' in this view.[9]

Now let us see how psychologists, wittingly or not, have used and confused the two sets of views. At the outset it seems that the problems for the psychologists are not concerned with the distinction between the epistemological and the ontological objects (*D* and *E*). Yet, except for slight differences in naming and emphasis, neither theory seems to have much bearing on any of the other distinctions. Psychologists have apparently, on the basis of these irrelevant metaphysical theories, attempted to dispense with one or another of the essential distinctions in the perceptual situation.

[6] See, for example, A. N. Whitehead's philosophic works; Alexander, S., 'Space, Time and Deity,' Macmillan, 1920.

[7] 'The New Realism,' various authors, Macmillan, 1912.

[8] As, *e.g.*, Poincaré, H., 'Science and Hypothesis,' N. Y., trans. by Lamor, J., 1905; Enriques, F., 'Problems of Science,' Open Court, 1914, trans. by Royce, K.; Pearson, K., 'Grammar of Science,' 3d ed., Macmillan, 1911.

[9] Perhaps the only outspoken acceptance of this view comes from Dunlap, K., 'Scientific Psychology,' St. Louis, 1922, and Hollingworth, H., 'Sensuous Determinants of Psychological Attitude,' PSYCHOL. REV., 1928, 35, 93–117, and 'Psychology,' N. Y., 1928.

The ill-named 'structural' psychologists [10] usually make all of the distinctions set forth in our analysis, and subscribe to the representative view of perception. The very problem of structural analysis, indeed, rests on the distinction between (*C*) what is sensed, and (*D*) what is meant or the perceptual object.[11] Observers need training because they do not distinguish in common sense between sensation and perception, (process and meaning, or content and object).

Titchener has named this confusion the 'stimulus error.' [12] This name has later been criticized by Bentley and again by Culler,[13] on the ground that the 'error,' if it is an error, for the inexperienced observer consists not in confusing what he actually observes (*C*) with the kind of physical energy affecting his sense organs (*E*), but with the perceived or meant object (*D*).

But I am not sure that Titchener was simply careless. That he is not entirely consistent with the thesis that perception is representative, but that he is sometimes quite positivistic in his interpretation, may be illustrated from his treatment of the subject matter of psychology and the various sciences. In the 'Textbook' he says,"Mankind . . . has sought . . . to give stability to a world of experience by assuming two permanent substances, matter and mind: the occurrences of the physical world are then supposed to be

[10] Titchener as early as 1898 ('The postulates of a structural psychology,' *Phil. Rev.*, 7, 449–465) pointed out that the structural account was incomplete, but expressed the conviction that the best hope for psychology lay in a continuance of structural analysis. Again, in 1912 he remarks, "Introspection can never give us a system of psychology" ('Prolegomena to a Study of Introspection,' *Amer. J. Psychol.*, 1912, 23, 447). The pupils and followers of Titchener are notorious for their disagreement with the extremeness of his position (except perhaps Weld, 'Psychology as Science'). See also Bentley's first two lectures in 'Psychologies of 1925.'

[11] This distinction is the burden of Titchener's paper, 'Description vs. Statement of Meaning,' *Amer. J. Psychol.*, 1912, 23, 165–182. In his 'Textbook' he attempts to emphasize the distinction by calling (*C*) dependent and (*D*) independent of the organism. In 'The schema of introspection,' *Amer. J. Psychol.*, 1912, 23, he calls (*C*) "an accurate description, made in the interests of psychology, of some conscious process" (486).

[12] Titchener, E. B., 'Experimental Psychology' II., ii, 1905, lxiii*f*, 203 etc., 'Textbook,' 1910, 202*ff*, 218. See also Boring's historical and critical account, 'The Stimulus Error,' *Amer. J. Psychol.*, 1921, 32, 449–471.

[13] Bentley, M., 'The Field of Psychology,' pp. 411*ff*, and Culler, E. K., 'The Stimulus Error,' PSYCHOL. MONOG., 1926, 35, (no. 2), 123–137.

manifestations of matter, and the occurrences of the mental world to be manifestations of mind. Such an hypothesis may be of value at a certain stage of human thought: but every hypothesis that does not accord with the facts must sooner or later be given up. Physicists are therefore giving up the hypothesis of an unchanging substantial matter, and psychologists are giving up the hypothesis of an unchanging substantial mind. Stable objects and substantial things belong, not to the world of science, physical or psychological, but only to the world of common sense" (15*f*). The subject matter of all the sciences is, then, not the common sense experienced world, but the world of human experience (pp. 2, 6). But note: psychology studies human experience as dependent upon the experiencing person; but the sum-total of human experience is always process, occurrence, and is dependent upon the nervous system and its attachments (p. 16). Place them side by side: experience is the subject matter even of the physical sciences: experience is process dependent upon the nervous system. Titchener surely does not mean to assert that botanists, for example, study nervous processes. In what sense then is experience the subject matter of the sciences? The answer is found on page 6: "Suppose that we take two points of view as far apart as possible, and discover for ourselves what experience looks like in the two cases. First, we will regard experience as altogether independent of any particular person: we will assume that it goes on whether or not anyone is there to have it." The word *particular* brings one to the second clause of the last sentence with the expectation that individual differences are to be ruled out; but there it is asserted that the nervous processes can go on independent of a nervous system! The term 'experience' is obviously being used in a double sense. It is used properly to denote experience as dependent upon a process set up in the nervous system by some stimulus, but improperly the term spreads over to include the object, and finally stands *only* for the data of experience, with the nervous system left entirely out. In this important section, at least, Titchener has confused the psychoneural event of observation (*B*) with the object perceived (*D*).

The mis-steps of some behaviorists have been pointed out and ignored so often that there is little excuse even for indicating where some of them stand with respect to the present analysis. The strict behaviorists characteristically attempt to dispense with several distinctions proposed in the analysis of the perceptual situation. I am not here interested in their whole-hearted rejection of awareness (A_2), because that has not led to many obscure confusions. It is their attempt to get rid of what actually is sensed (C) that calls for mention. On the one hand they adopt a common sense realism, ignore the relativity of perception, and consider what actually is sensed as a part of the physical object; and on the other hand, they consider what actually is sensed to be the sensory discrimination (B) and identify the latter in turn with some sort of motor response. The situation is further complicated by professions of faith in the tenets of representative realism. Thus, the half dozen uses of the term 'external world' found in Weiss's book [14] which have elsewhere been pointed out [15] may be reclassified under the three heads, as follows: (1) the stimulus is called a metaphysical fiction (p. 44), and 'my response' (23, 43). The use of this terminology might be considered merely an attempt to emphasize the fact that our knowledge of the external world is not certified, or an attempt to state solipsism behavioristically, were it not so fundamental to behaviorism.[16] The remaining treatments, however, imply on the one hand the causal or representative view, and on the other, the direct view of perception. (2) The section on the nature, transformations and conservation of energy (chap. 1), and the first four 'postulates' consider the ultimate nature of the world to be electronic. (3) But this view seems to be

[14] Weiss, A. P., 'A Theoretical Basis of Human Behavior,' Columbus, 1925.

[15] Gundlach, R. H., 'Some Difficulties with Weiss's Behavioristic Postulates,' *Amer. J. Psychol.*, 1927, 38, 470*ff*.

[16] Hunter affords a recent example: "The subject matter of anthroponomy is behavior. By *behavior* is meant *the muscular and glandular activity of the organism*." (p. 2). Yet he has two chapters concerning 'receptor process,' and he appears to differentiate these things not in terms of muscles and glands, but rather after the manner of the 'structuralist.' I also wonder, parenthetically, what can be meant by such phrases as this: "and one's hand may withdraw itself upon the appearance of an idea of a painful object" (p. 47). Hunter, W. S., 'Human Behavior,' Chicago, 1928.

discarded for the direct view of perception with the statement: "These [the stimulating conditions] may be as simple as that of a loud sound, or as complex as that of an archeological record" (p. 323, see also pp. 41, 272, 340). The assertion that the elementary discriminative responses in combination make up the objects and events of the environment (p. 199) differs perhaps only verbally from the assertion of the new realists that an object is a collocation of 'sensa' with this exception: the philosophers usually distinguish the event and the data of observation, while the behaviorists often fail to make this distinction.[17]

Certain other psychologists may be called 'mental behaviorists' since they differ from the strict behaviorists principally in that they insist upon awareness in some sense. It may be well to discuss in this connection Knight Dunlap who, it seems to me, has misinterpreted the entire history of modern philosophy by failing to understand the representative theory of perception.[18] The traditional view has stated, I think, that there is an actual independent material (atomic) world which affords the energy for the stimulation (E) of our sense organs, and that our perceptions (D) are derived from these sense data (C). Dunlap converts this theory into the naively realistic view that the observed objects (D) are all that is; and then puts forth the interpretation that these philosophers and their present psychological relatives have fancied an identical 'parallel,' 'psychic' world, and interposed it between ourselves and our perceptions. He expounds the naive view when saying, for instance, that "so far as our knowledge goes, physical stimuli are mathematical expressions

[17] The behavioristic scheme fits much better into naive realism because the latter is devised to make possible the consideration of 'sensa' as inherent in the physical object. But the behaviorist usually holds to that point of view only long enough to make a jab at traditional psychology. For example, as Hunter says, "The student of mind either assumes mysterious elements which cannot be verified by science or else calls the inner and outer environment psychic processes. No one denies that the world is composed of colors, tones, stones, trees, and other similar objects" ('Human Behavior,' 1928, p. v, and again in his lecture in 'Psychologies of 1925'); and yet even he speaks of wave lengths and receptor processes.

[18] In his first lecture, 'Psychologies of 1925.' The same view is more compactly stated in his 'Elements of Scientific Psychology,' St. Louis, 1922.

and nothing more. In other words, they are expressions, in terms of mathematical conventions, of complex relations and systems of relations subsisting between sense data." [19] Further, "introspectionists assume that the observable world is double in form; or may we say that there are two objective worlds. One of these is the physical world: the other a quite distinct psychical world. Between these two worlds there is supposed to be a *correspondence* such that for each object in the psychical world there is an object, or a complex of objects in the physical world. Both, however, are assumed to be directly observable and to resemble each other so much that it is quite possible, when attempting to observe psychical objects, to observe physical facts instead. . . . This alleged confusion is called the 'stimulus error.'" [20] The representative view, however, does not hold that physical stimuli are nothing but mathematical expressions, but holds that the atom is an actual physical entity, although of course it is not perceptible. On this view then there are not two observable worlds corresponding so completely that they can with difficulty be distinguished.

In summary, then, the distinction between sensation and perception must be made even by naive realists.

II. *The distinction between subjective and objective observation.* Although the different metaphysical positions that have been outlined have themselves little bearing upon the psychological problem of perception, they do affect the present problem. The traditional psychologists following the representative view have little use for the distinction. Since the organism exists in an atomic world, and sense organs and nerves all have their own (modified) specific energy, there can be little distinction between the observation by one sense, and the observation by another. The problem of inspective observation consists in reporting what actually is sensed.[21] Bentley differentiates two other types of report, the account

[19] 'The Elements of Scientific Psychology,' p. 43.

[20] *Op. cit.*, pp. 33*f*.

[21] Cf. Titchener, E. B., 'A Beginner's Psychology,' 1915, sec. 5; Washburn, M. F., 'Introspection as an Objective Method,' PSYCHOL. REV., 1922, 29, 89–112; Bentley, M., 'The Field of Psychology,' chap. 2.

of the outcome of some performance, or terminal report, and the comments about the situation.[22] Followers of the new realism, however, have a good use for the subjective-objective distinction in differentiating those perceptions of objects which are 'public,' and the perceptions of objects and events which are going on within their own organisms.[23] They very often fail to distinguish the problem of inspective observation (the account of *C* as opposed to *D*). Most of the confusions about introspection have been due to the fact that the behavioristic critics have not usually gone to the individuals who were supposed to have used the method for their definition, but rather to such 'mental behaviorists' as Dunlap or Woodworth, who distinguish subjective as visceral, and objective as exteroceptive.[24]

III. *Problems of mind and body.* One constantly encounters the behavioristic announcement, despite most arguments to the contrary, that the only alternative to dualism is physical monism; and that any suggestion of 'mental' characteristics implies an insidious dualism. On the other hand the dualistic conception has certainly dominated most of the traditional psychologies. But the possible mind-body views are numerous. The probable views are by no means limited to the traditional ones held in psychology, and in fact, the most promising interpretations seem but little known in that field. I shall list the various views which, after Broad's analysis, seem possible and as the emergent views seem to play into the hands of the unsuspecting gestalt psychologies, I shall present that account rather more elaborately.

A brief glossary will make the outline more comprehensible:[25]

1. *A differentiating attribute* is one which serves to determine the kind or nature of a substance; *e.g.*, a material or

[22] 'The Major Categories of Psychology,' PSYCHOL. REV., 1926, 33, pp. 88*ff*.

[23] *Cf.* Dunlap, K., *op. cit.*, 27*f*, and Woodworth, R. S., 'Psychology,' p. 9.

[24] Representative treatments may be found in Smith and Guthrie, 'General Psychology,' 1921, p. 244; Weiss, A. P., 'A Theoretical Basis of Human Behavior,' Adams, 1925, pp. 70*f*; Dashiell, J. F., 'Fundamentals of Objective Psychology,' Houghton Mifflin Co., 1928, pp. 11*ff*.

[25] A very large part of the material, and most of the manner of presentation is taken from Broad, C. D., 'The Mind and its Place in Nature,' Harcourt, Brace & Co., 1925.

mental substance. It is not essential to Substance as such; it is itself a determinable but is not determinate under any higher determinable; it belongs to all of the parts; it is simple.

2. *An emergent attribute* belongs to a complex as a whole, but not to its parts.
3. *A reductive attribute* belongs to all the parts as well as the whole, and is not emergent.

Items (2) and (3) actually apply to certain things in the universe.

4. *A delusive attribute* has no application. There are no things in the universe which have these attributes in some determinate form, although they seem to apply.
5. *Mentality* (mind) is an attribute which involves at least feeling and perhaps acquaintance or perceiving.
6. *Materiality* (body) is an attribute which involves at least duration, extent, persistence, position, etc.

We are now ready to indicate the possible relationships between mind and body. To develop an exhaustive classification of the relationships which may hold between these two attributes, it is necessary to list all the permutations and combinations of mentality and materiality among the four alternatives, differentiating, emergent, reductive, and delusive.

I. Both mind and body are differentiating attributes.
   A. Dualism of incompatibles: mind and matter cannot both be characteristic of the same substance.
      1. Interaction (dual causal relations).
      2. Parallelism (no causal relations).
      3. Epiphenominalism (one-sided causal relation).
      4. Hypophenomenalism (the other-sided causal relation).
   B. Dualism of compatibles: mind and matter can belong to the same substance.
      5. Dual aspect view.

II. Either mind or matter is a differentiating attribute. The view is named for the differentiating attribute.

A. The other is delusive.
6. Pure mentalism (idealism).
7. Pure materialism.

B. The other is reductive.
8. Reductive mentalism.
9. Reductive materialism, (biological mechanism).

C. The other is emergent.
10. Emergent mentalism.
11. Emergent materialism.

III. Neither is a differentiating attribute.[26]

A. Both are emergent, or both are reductive.
12. Emergent neutralism.
13. Reductive neutralism.

B. One is emergent; one is reductive.
14, 15. Mixed neutralism.

C. One is delusive; the other is emergent or reductive.
16, 17. Mentalistic neutralism.
18, 19. Materialistic neutralism.

D. Both are delusive.
20. Pure neutralism.

Broad discards a number of the views as demonstrably false and leaves the issue unsettled between the remaining few. It will suffice here to indicate the general tenor of the emergent concept by comparing emergent materialism and reductive materialism with respect to the difference in behavior between things.[27] The differences in behavior between different things

[26] Thus: view 7 would assert that materiality is a differentiating attribute, and mentality is delusive. There seems to be a possible confusion in Broad's terms referring to the views listed as 9 and 11. In outlining his seventeen types of theory (pp. 609–911), he uses the terms in the present sense; but in his early discussion of mechanism and its alternatives (Chap. II., esp. pp. 60, 69*ff*) he calls reductive materialism 'biological mechanism.' and emergent materialism 'emergent vitalism.' The former is consistent in that the name indicates that materiality is the differentiating attribute. Emergent vitalism, however, should indicate that 'vitality' was the differentiating attribute; but this obviously is not what he means.

[27] See Broad, C. D., *op. cit.*, pp. 58*ff*. Various accounts of the emergent view may be found in Alexander, S., 'Space Time, and Diety,' London, 1920; Haldane, J. S., 'Mechanism, Life, and Personality,' London, 1921; Sellars, R. W., 'Evolutionary Naturalism,' Chicago, 1922; Morgan, C. L., 'Emergent Evolution,' London, 1923; Smuts, T. C., 'Holism and Evolution,' London, 1926; and Wheeler, W. M., 'Emergent

are not due to some peculiar component (such as the theories of entelechies supports) but to differences in structure. On the emergent view, however, "The characteristic behaviour of the whole *could* not, even in theory, be deduced from the most complete knowledge of the behaviour of its components, taken separately or in other combinations, and of their proportions and arrangements in this whole." [28] So far as we at present know, the characteristic behavior of common salt can be learned only by studying common salt; it cannot be deduced from a knowledge of the elements in isolation, or in other combinations. On the reductive view "the characteristic behaviour of the whole is not only completely *determined by* the nature and arrangement of its components; in addition to this it is held that the behaviour of the whole could, in theory at least, be *deduced* from a sufficient knowledge of how the components behave in isolation or in other wholes of a simpler kind." [29] Obvious examples of this type of structure are artificial machines.

We cannot here weigh the evidence for these two views. But let us assume that our chemical elements and compounds are *not* emergent: would there be any theoretical limit to the deduction of the properties on the mechanistic theory? Broad holds that there would be: that an archangel could deduce from his knowledge of the structure of atoms all the facts about the combination say, of nitrogen and hydrogen, except such a thing as its odor. "The utmost that he could predict on this subject would be that certain changes would take place in the mucous membrane, the olfactory nerves and so on." [30] "There is the group of microscopic particles on

Evolution and the Development of Societies,' 1928. Wheeler devotes a part to an account of some early and contemporary organicists, as he calls the view. He traces the notion from the early 18th century in the expressions of various neurologists, biologists and psychologists. Although Wheeler does not mention James, a good deal of the prevelance of the view among philosophers and psychologists in this country, at any rate, may be traced to him. See, for example, 'Psychology,' Chap. VI.; 'A Pluralistic Universe,' pp. 185*ff*; 'Pragmatism,' pp. 138*f*, 257, 287–292; 'Problems of Philosophy,' pp. 45*f*, 132, 140*ff*, 166–173. One could also add to the list of contemporary scientists, Child, C. M., in 'The Origin and Development of the Nervous System from a Physiological Viewpoint,' Chicago, 1921, esp. Chap. I.

[28] Broad, 'The Mind and its Place in Nature,' p. 59.

[29] *Loc. cit.*

[30] *Op. cit.*, p. 71.

the one hand, with the fact that the volume which contains the group is, or appears to be, pervaded by such and such a secondary quality." [31] The archangel could predict the odor well enough if he had ever smelt such a compound before. Although odors, colors, sounds may be functions of, or dependent upon receptors and the nervous system, a complete knowledge of the physics and chemistry of response in sense-organ and nerve would be no substitute, among the anosmic, blind, or deaf, for the direct experience. There is, obviously, no logical connection between a certain stimulus and a certain sense datum, but simply a factual one that must be discovered by smelling, looking, etc. There is no a priori reason why one group of wave lengths should be the stimulus for heat, another for sound, and a third for colors. Thus Broad holds that the appearance of sensory qualities in the world demands that the reductive laws be supplemented—if at no other point —by laws of the emergent type.

It seems, further, according to Broad that so long as we regard simply the behavior of the organism as a going concern, there is little to differentiate between the reductive and the emergent views.[32] But the case is altered if we sider the origin of organisms. Since reductive mechanism is admittedly a theory of the organism based on the analogy of self-regulating machines; and since these so far as we know could not have arisen by themselves, but only by the design and interference by someone with matter, the theory seems driven to an analogous interpretation of the origin of organisms. Organisms may have arisen out of non-living matter, due to the deliberate intervention of some designer who arranged this matter into the form of a reproducing natural machine. The only alternative to this Deistic origin would require a series of organisms (natural machines) whose ancestry goes back to infinity. The emergent theory, however, as its name implies, rests on the possibility of the development of new characteristics and properties in combinations, not found in the isolated constituents. The qualities

[31] *Op. cit.*, p. 80.

[32] On the other hand, both Wheeler (*op. cit.*), and Child (*op. cit.*) attempt to demonstrate cases of emergence or novelty in behavior.

and properties of life, mentality, and social organization could develop or emerge from a universe of inanimate matter. The emergent theory would assume "a general tendency for complexes of one order to combine with each other under suitable conditions to form complexes of the next order. At each stage in this progress we shall get new things with new and irreducibly characteristic properties and new intra-ordinal laws, whilst there will probably remain complexes of all the lower orders. The universe would thus grow continually more varied, so long as the special conditions necessary for this combination of complexes of lower order to give complexes of higher order continued." [33] One of the crucial differences, I think, between the mechanistic and the emergent views centers about causation. The reductive mechanist holds to the Aristotelian principle that the result cannot exceed the cause. Hence in his explanation of origins, he must posit, either the actual existence of all the characteristics which now have or will be discovered (or developed), in every bit of matter; or the creation of some properties by a Deity. The emergent materialist on the other hand, rejects the ancient and scholastic view of causation, and holds that a totality may have properties other than, or additional to, those obtained by merely summing the constituents.

This section may be summarized not so much in terms of body and mind as in terms of mechanism and its alternative views:

There is *A. Mechanism*, which holds as its theoretical ideal the reduction of the laws of behavior of everything to some simple law or set of laws. These were formerly held to be the laws of mechanics, and are now usually held to be the laws of electricity. The anti-mechanistic views include:

B. Vitalism, of the entelechy variety, which explains theoretically the behavior of complex organisms by the addition of some new element to the situation not found in the mechanism of the organism.

C. Emergent materialism, or the creative synthesis view, which holds that some totalities may be more than the sum

[33] *Op. cit.*, p. 93.

of their parts; that each new level of emergence must be studied itself before its laws can be ascertained.

IV. *Scientific methodology.* This section refers more particularly to the recent tendency of the Columbus behaviorists to build up a system of psychology upon a set of postulates and axioms.[34] The matter is worth general discussion however in view of the fact that so many other writers often try to settle problems of fact by stating that they 'do not admit that distinction,' or that 'with our point of view the problem vanishes.' It appears to me that these psychologists view the contemporary situation as a set of different games of cards. The bridge player, accordingly, pays no attention to the poker addict who suggests that he raise the ante. They are not 'talking the same language.' More seriously, I believe that Weiss and his followers have attempted to follow the procedure of mathematics: they take a few indefinables such as movement and position, lay down a set of postulates, and attempt to build—not too rigorously—a system of behavioristic psychology. Now it also appears that for the founders, the test of the system is the consistency with which other concepts can be interpreted in the behavioristic language; and its justification, the pragmatic success with which certain problems can be treated. Stated briefly, these psychologists are attempting to follow the methods of mathematics in a natural science; and are justifying the outcome of their theories not on the basis of a correspondence between theoretical consequences and observed facts, but on two scores, the self-consistency of the terminology, and the development of an experimental program. This is a method which in a natural science, of course, tends to settle opinion about matters of fact before the facts have been thoroughly investigated. Since the method is not strictly carried out, I do not believe these psychologists recognize that their methodology tends to be of this sort.

The discussion can be made clearer by distinguishing two, and perhaps three uses of the term 'postulate.' The mathe-

[34] *Cf.* Weiss, A. P., *op. cit.*, also 'A Set of Postulates for Social Psychology,' *J. Abn. & Soc. Psychol.*, 1926, 21, 203–211; Rexroad, C. N., 'A Formulation of the Practical Assumptions Underlying Psychology,' Psychol. Rev., 1927, 34, 116–119.

matical logicians use it in one sense. If I understand rightly, the procedure in this field is somewhat as follows: a few *undefined ideas* are taken, and several *postulates* are stated in terms of these ideas. From this groundwork are deduced by formal logic the various theorems and corollaries of the system. Finally, the postulates may be shown to be *sufficient*, *consistent*, and *independent*. The system of theorems will be true of any group of entities which as a matter of fact have just these postulated properties.[35] The mathematician is not primarily concerned with actual existence of natural entities having the characteristics described, but rather with the *formal* properties of the hypothetical entities. This is a basic difference between logic and science, for it is one of the main tasks of the scientist to discover what sort of things actually do exist. Let us consider postulates and the sciences.

The term ultimate postulate has good usage out of mathematics in the general problem of knowledge. According to an analysis by Broad, ultimate concepts are propositions about facts which are not self-evident, nor deducible from some other propositions: they are not directly observed, nor inferred from observation (the attempt to prove 'uniformity' of nature inductively is circular); they cannot be refuted by argument, and finally, in practice everyone assumes them.[36] These ultimate postulates have usually included the assumptions (1) that every change has a cause, (2) that one's memories are in part at least valid, and (3) that the 'testimony' or evidence offered by other people is partially reliable. These postulates are necessary if any unification of our knowledge is attempted; but they are not necessary for knowledge by direct acquaintance. We observe what we actually do observe (*i.e.*, 'sensa'), although our perceptions of objects and events, since they involve a

[35] *Cf.* Young, J. W., 'Fundamental Concepts of Algebra and Geometry,' N. Y., 1911, and Richie, A. D., 'Scientific Method,' N. Y., 1923. In this connection it may be well to mention the euclidian and various non-euclidian geometries, or the deduction of the theorems of algebra by Whitehead and Russell, as examples of the method of mathematical logic.

[36] Broad, C. D., 'Critical and Speculative Philosophy,' 'Contemp. Brit. Phil.,' 1st series (ed. by Muirhead), Macmillan, 1924, pp. 85*ff*. See also 'Mind and its Place in Nature,' Section B; and Santayana, G., 'Skepticism and Animal Faith,' N. Y., 1924.

reference, may be illusory.[37] No postulates are necessary, however (as certain behaviorists have held in discussion), for the apprehension of such things as movement, or of odors. What our postulates hold is that our apprehensions (perceptions, memories, etc.) are not usually illusory. The objects of our perceptions provide the data, the facts for the sciences; and our perceptions, buttressed by the postulates, can be taken as true in some sense. Now the scientist proceeds to pick out certain uniformities in his observational field and to generalize them into laws. A scientific law states what can be observed to happen under given conditions, other things being constant.

There has yet been no need for additional postulates in either of the above senses. But the next step in scientific methodology introduces what I believe to be at times a misleading, or even false usage of the term. The next step is the attempt to explain, or to organize under a more abstract formula, the empirical laws. The explanation is put forward tentatively in the form of an hypothesis or theory which attempts to describe the non-perceptual elemental constituents which form the antecedents of the observed uniformities expressed by the law. (The elements may be actual atoms, etc., or only the mathematical expression, and the 'as if'.) In forming his hypothesis the scientist simply uses his 'hunch': but in working it out he uses his mathematical logic. He says, in effect, that if we have a system of such and such hypothetical entities, with such properties, then certain (observable) events should follow. Now, since we can demonstrate some of these consequences we can infer, with some probability, that there may be a system of actual entities such as those hypothesized. The theory is thus on its way toward verification. But another set of assumptions may fit more of the data: or certain consequences may be incompatible with the first theory, and so revision and further testing follow. The term 'postulate' has at times been applied, often with innocent enough consequences, to the theoretical constructions which the data seem to demand.

[37] Here again we have the distinction between 'sensation' and 'perception.'

But now, the demands of the theory are properly stated as hypotheses, assumptions, guesses, hunches which have a certain probability of truth, and possibility of verification.

It is strange enough that the term 'postulate' could ever have been used as synonomous with 'guess' and the like; but stranger still is the burden the behaviorist puts on this language. He makes out a set of assumptions for the behavioristic theory, but appears fearful of the consequences of experimental testing, and throws up a verbal Chinese Wall about them: he calls them *Postulates*. The theories which form the basis of the behaviorist's experimental program are never tested in the light of old or newly discovered data. Rather, they are accepted as axiomatic, and are considered to be a sufficient test of the very existence of facts. This method as I have said, is directly analogous to the methods of the formal science of mathematics and is completely inapplicable if not fallacious when used in natural science. For the mathematician, facts which do not follow deductively from the postulates formulated to define a specific type of system are arbitrarily rejected. Such facts are not denied existentially by the logician, but are irrelevant since they do not embody any values for the functions with which he is concerned. The difficulty which I find with Weiss's method is just that he attempts to use his scientific assumptions as arbitrary standards of the existential status of facts. If he would admit that his assumptions restrict him to a certain limited portion of the data of psychology, and cut off from him, *e.g.*, analytic facts of experience, or the facts of configurations, there could be no quarrel. But when his system becomes a 'theoretical basis of human behavior,' and upon such a basis the behaviorist tacitly denies as illicit, or damns as 'mystical' any evidence contrary to his 'postulates,'[38] he has gone farther than the method permits. This postulatory method gains a specious and high-sounding plausability for a set of untested guesses by the simple and time-worn device of attaching to it a name to which scientists have already de-

[38] This is the burden of P. T. Young's example of an account of the world denying the existence of black cats: 'The Phenomenological Point of View,' PSYCHOL. REV., 1924, 31, 292*f*.

veloped a somewhat worshipful attitude. It appears to be as successful, moreover, in certain quarters as the demagogue's use of such terms as *mother*, *bolshevik*, *home*, *slacker*, and *the flag*, in starting a lynching bee or getting recruits. But this type of 'postulatory' method has no place in any natural science.

The term 'postulate' has now been used in three different contexts: as ultimate postulate, as a starting point for deductive logic, and as the scientist's guess about what he cannot observe. The third is probably an application of the second usage, the difference being that the mathematical logician theoretically cares nothing about actual systems in the world, and his postulates are made in entire disregard of what is existent; while the scientist's postulates are solely about what exists. One might say that the scientist's postulates are either true or false, while the mathematical logician's postulates are existentially neither. Now in formation of scientific hypotheses, assumptions are certainly essential: but they are the questions, not the theoretical basis of a science. Their fruitfulness lies not in providing a believer with a bulwark for his faith, but in providing an inquirer with live possibilities which may be tested, and proved or rejected.

The discussion has so far considered only the method of postulation as applied to psychology as a natural science. Another sort of objection arises with respect to the specific nature of the Weissian postulates. The 'physical' postulates appear to me not only false according to modern physics, but irrelevant. What difference would it make in Weiss's system, for instance, whether euclidian or non-euclidian geometry were true of the world? What difference would it make if the atom and its systems was not the hard little pellet having a history in time and space which he describes, but a four-dimensional space-time event of modern physics: whether it were the Bohr atom, the Heissenberg atom or some other type of atomic structure?[39] If what these behaviorists are attempting to assert is simply a faith in the unity of Science,

[39] For good accounts of modern physics in less technical form see, *e.g.*, Russell, B., 'The Analysis of Matter,' N. Y., 1927, and Huntington, A. S., 'Space, Time, and Gravitation,' Cambridge, 1921.

they could have said just so much, simply and safely. But then their faith would not differ from that of most other psychologists. On the other hand, the 'psychological' postulates are certainly not universal or ultimate; nor do they provide explanatory hypotheses for observed uniformities. They simply name the sort of problem held to be important.

In principle, then, I have tried to show that the postulatory method as used by certain behaviorists is not suited to establish one theory over another, or to snub any matter of fact. The sort of difficulties into which the use of this method plunges one is illustrated by one consequence of the assumption of 'physical monism.' (Probably Weiss does not mean pure materialism, which is certainly false, but reductive materialism). As we saw in the previous section there are a good number of different views that may be true, and the reductive view can perhaps treat, theoretically, only the electronic or atomic combinations. The solution of these problems has been the task of the physicist and chemist. Sense data, however, (secondary qualities) are not atomic structures, but are on an entirely different level, and probably require laws of the emergent type. The behaviorists, to forestall such a possibility, are driven to deny sense data as dependent upon the apprehending organism,[40] and are thereby forced to remodel their atomic 'physical monism' by putting the 'qualities' into the environment. Then the matter of illusions, hallucinations, perspectives, etc., which distinguish sensation from perception comes up again, and we are once more launched upon the problems of representative and naive realism discussed above in the first section.

Summary

This paper has attempted to show:

1. That there are at least two classes of metaphysical

[40] This is especially evident in Hunter ('Human Behavior,' pp. 14*f*) where he holds that "the only relationship between the experimenter and the behavior which he is observing is one of stimulus and response. The experimenter is stimulated by the subject who is behaving and often by the stimulus which causes the subject to behave." The question, however, is about the nature of observation, which Hunter, of course, would not confuse either with the energy pattern of the stimulus, or with the notations on paper—the relatively permanent "subject matter for the science."

theory about perception. (*a*) But that being theories, they do not differ as to the facts of perception, but differ in the interpretation of those facts; and (*b*) that certain psychologists on the basis, apparently, of one or both of these theories have played fast and loose with facts, confusing certain of the distinguishable features of perception.

2. That the method of 'introspection' as criticised by most behaviorists is usually not the method typical of the 'introspectionists,' but a type of observation attributed to them by the 'mental behaviorists.'

3. That mechanism and the entelechy variety of vitalism are not exhaustive alternatives, since there are in addition the emergent views.

4. Finally, that the postulatory method is not applicable to a natural science.

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MATURATION AND INFANT BEHAVIOR PATTERN[1]

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The influence of conditioning on the human infant has been so forcibly asserted from the standpoint of behaviorism, that it may be desirable to examine the influence of sheer maturation on his patterns of behavior.

The behavior of the infant, by nature, is obedient to pattern. Never does the picture of normal behavior become as diffuse and formless as a drifting cloud. Even the random movements of the month-old child are not utterly fortuitous. The closer one studies them the more configuration they assume. There is indeed no such thing as utter randomness in infant behavior. Accordingly the 'random activity' of the two months infant is distinct from that of the month-old. It is distinctive because it has its own pattern.

Likewise with the fœtus. Its behavior is in no sense amorphous, but, as the studies of Minkowski have shown, manifests itself in fairly well-defined reflexes—long, short, diagonal, and trot reflexes; postural reflexes; rhythmical and inhibitive phenomena. These patterns of behavior follow an orderly genetic sequence in their emergence. Genetic sequence is itself an expression of elaborate pattern. And the relative stability of both prenatal and postnatal ontogenesis under normal and even unusual conditions must be regarded as a significant indication of the fundamental rôle of maturational factors in the determination of behavior.

Again take the fœtus. The uterus is the normal environment of the fœtus till the end of a gestation period of 40 weeks. But birth with survival, may exceptionally occur as early as 24 weeks and as late as 48 weeks, an enormous range

[1] Paper presented at the Thirty-Seventh Annual Meeting of The American Psychological Association, New York City, December 28, 1928.

of variation in natal age amounting to 6 lunar months. Variation with a range of 3 lunar months is common and yet this considerable variation does not impose a corresponding deviation on the complex of behavior. Our normative studies of both premature and postmature infants have shown repeatedly that the growth course of behavior tends to be obedient to the regular underlying pattern of genetic sequence, irrespective of the irregularity of the birth event. Refined studies will doubtless reveal that such irregularity does subtly modify many details of behavior; but as a point of departure for the discussion of maturation, nothing is more comprehensive in implication than the general stability of the trend and the tempo of development, in spite of precocious or postponed displacement of birth. The patterns of genetic sequence insure a basically similar growth career for full term, pre-term, and post-term infants. It is as though Nature had provided a regulatory factor of safety against the stress of extreme variations of environment. In the mechanisms of maturation this regulation operates.

The term *growth* may be construed to embrace the total complex of ontogenetic development. Maturation refers to those phases and products of growth which are wholly or chiefly due to innate and endogenous factors. It is our purpose to assemble in a summary manner, diverse evidences of behavior maturation, based upon our clinical, experimental and normative observations.

These evidences are drawn from several sources as follows:

(1) The development of prehension.
(2) Developmental correspondence in identical twins.
(3) The limitations of training.
(4) The restricted influence of physical handicap.
(5) Developmental progression in emotional behavior.

1. *The Development of Prehension.*—The development of prehension throughout the first year of life displays significant progressive changes in behavior pattern. These changes raise searching doubts concerning the influence of experience and training upon these patterns. We have studied these changes with particular reference to a pellet 8 millimeters

in diameter. The characteristic eye-hand reactions of an infant confronted with this tiny pellet may be recapitulated in the following which is a genetic order:

(*a*) No visual regard for the pellet.
(*b*) Transient regard for the pellet.
(*c*) More prolonged and definite fixation upon the pellet with slight postural changes (16 weeks).
(*d*) Visual fixation with crude bilateral or unilateral hand approach (20 weeks).
(*e*) Unilateral pronated hand approach with scratching in vicinity of the pellet (24 weeks).
(*f*) Pronated hand approach with occasional raking flexion resulting in palmar prehension (28 weeks).
(*g*) Pronated hand approach with extension of index finger and partial suppression of other digits resulting in poking or prehension by index finger with partial thumb opposition.
(*h*) Rotation of wrist in hand approach, with pincer-like prehension of pellet by index finger and thumb (40 weeks).
(*i*) Perfection and further delimitation of pincer-like response.

All these changes mature with subtle but significant accompanying changes in head posture, body posture, hand and arm attitude and associated visual behavior. It seems quite erroneous to say that the child learns to prehend the pellet in the traditional sense of the learning process. Crudely, but nevertheless effectively, he prehends the pellet by gross palmar approach as early as the age of 28 weeks. The refinement of his eye-hand behavior comes not by the alleged utilization of snatches of successful random activity, but by the progressive acquisition and consolidation of a hierarchy of behavior patterns which are the result of developmental decrements and increments rather than the stamping in or chaining of satisfying, successful reflexes. The defective child shows retardation in the acquisition of these patterns even though he may, in a durational sense, have a larger fund of prehensory experience. It is not improbable that many of

these developmental changes in the pattern of prehension would be realized even if the prehensory hand were altogether swaddled and deprived of activity. When the prehensory mechanism is damaged by restricted birth injury to the brain, resulting in extensive athetosis, the propensity to prehend or reach may still assert itself at the proper genetic level. Even though the propensity is aborted its presence is highly suggestive of the potency of maturational determination.

2. *Developmental Correspondence in Twins.*—During the past year we have gathered extensive detailed data on the development of prehension in a pair of identical infant twins. These twins were identical not only with regard to their skin patterns but also to a remarkable degree, with regard to their behavior patterns. Nowhere was this more objectively shown than in their prehensory reactions to cubes and pellets under controlled observational conditions. At 28 weeks both of these twins, being somewhat retarded in their development, were visually unheedful of the pellet, though they definitely regarded a cube. At 38 weeks they addressed themselves in an identical manner to the pellet. The hands were in full pronation, the fingers, spread apart in a fan-like manner, were fully extended. The thumb was fully extended almost at right angles. The photographic record of their attack upon the pellet, in the motion pictures, shows an almost uncanny degree of identity in the details of postural attitude, hand attitude and mechanism of grasp. Time does not permit the further specifications of these details.

At 40 weeks each twin made a crude raking attack upon the pellet, with occasional awkward but completed prehension in which the palm and all of the digits participated. The form of the prehension pattern was again remarkably similar in the two children. At 42 weeks they were again examined in the same situation. Although there had been no special instruction or conditioning in the interval, these 2 weeks imposed a palpable and strikingly similar change upon the prehension picture. Simultaneous flexion of the digits was very neatly displaced by a preferential flexion of the index

finger. The raking approach was replaced by a poking with the tip of the index finger. Such an interesting inflection of the prehensory pattern surely could not have been induced so precisely and so simultaneously in both of these children without the presence of controlling factors of organic maturation. Of similar significance is the fact that comparable changes in prehension pattern appeared coincidentally throughout the course of their development.

The correspondences in behavior patterns in these twins were literally uncountable. However, the records of 13 developmental examinations were analyzed and 612 separate comparative ratings of behavior items were made from these records in order to determine items of correspondence and disparity. There were 99 items of minor disparity and 513 items of identical or nearly identical correspondence. The parity of behavior patterns was overwhelming.

Many convincing examples of behavior correspondence might be cited. We content ourselves with a few much abbreviated illustrations. Here is one which seems to us to have experimental control, even though it deals with nothing more than the reaction of two infants when placed in exactly the same manner upon a flat platform to observe their postural control in the sitting position. Both children showed precisely the same kind and degree of difficulty in equilibrium at the age of 28 weeks. In both there was a tendency to sway to the right; in both it was impossible, even by spreading the legs, to make the body lean forward sufficiently to establish a passive balance. In the case of each child there was an antagonistic tension which made the body rebound backward in an automatic manner resembling a sharp spring-like action of a knife blade snapping into position. We have never seen precisely this kind of reaction in an infant at this age. It is inconceivable that the response arose out of some identical conditioning factor in the environment. It is reasonable to suppose that this distinctive behavior pattern reflected a maturity level and a synchronous neural organization shared by both children because of their common genetic origin.

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Within a week this reaction disappeared. Four days later the twins were placed upon a large blotter on the platform of a clinical crib and maintained the sitting position by leaning forward. Simultaneously they attacked the blotter with the hand in full pronation, and simultaneously, with vocalization, they continued to scratch the blotter, leaving visible marks. Here again was a dramatic bit of correspondence all the more impressive because displayed simultaneously. The complexity and nature of these two behavior patterns again suggest the determining rôle of maturation. If it is argued that extrinsic factors determine the form and the time incidence of these simultaneous patterns it is necessary to demonstrate in detail the cunning arrangements of environment and of conditioning stimuli which could design so precisely, and in duplicate, the configuration of behavior. How can the environment, even of twins, accomplish such architectonic miracles?

A brief example of behavior correspondence may be cited from the 44 weeks examination record. The twins were confronted with a test performance box with its three holes. The common method of approach of the two children, their preferred regard for the edge of the performance box, the fleeting regard for the holes, the exploitation of the vertical surface of the performance box by a scratching, simultaneous flexion of the digits, the failure to place a round rod into any of the holes, the brushing of the surface of the performance box with the rod, the transfer of the rod from one hand to the other, and finally an almost simultaneous peculiar, clicking vocalization in both twins—altogether constituted a very complicated behavior pattern, but one which bristled with numerous identities of spatial and dynamic detail. One can give due weight to the significance of this correspondence only by reflecting on the myriad of behavior exploitations of the situation which the twins *might* have adopted. But in spite of this multitude of exploitational possibilities, the twins were apparently under a common inner compulsion to adopt those very similarities of behavior which have been noted.

Still another, and very pretty example of identity was disclosed in the pellet and bottle test at 48 weeks. This test involved a bit of learning as well as perception and prehension. Three trials were made with each child. The examiner dropped a pellet into a small glass bottle and then gave the bottle to the child. Both children watched the dropping of the pellet with the same transfixed attention. Both children, on the first trial and on the second trial too, seized the bottle apparently heedless of the contained pellet. Both children on the third trial pursued the pellet by poking at it against the glass. Here the details of behavior pattern extended even into the marginal zone of adaptation through learning.

In passing it should be noted that although these observations on twins are comparative, they are objective. They have an objective, quantitative validity. It must be insisted that it would be very difficult to devise a more complicated and in some senses a more delicate instrument of behavior measurement than one twin used in juxtaposition with an identical co-twin as a standard of reference and comparative observation.

3. *Limitations of Training.*—While the positive results of training and conditioning have somewhat obscured the factors of maturation, the limitations of training may be adduced to show the existence of these factors. Such limitations were put to experimental study [2] in the same pair of twins (Twin T and Twin C) whom we have just cited. At the age of 46 weeks, when the thorough-going mental and physical identity of the twins had been well established, it was decided to determine the influence of training confined to one twin, by using an experimental method which we have designated *the method of co-twin control.* T became the trained twin; C was reserved as a control.

Very briefly, Twin T was systematically trained for 20 minutes daily over a period of 6 weeks, in two fields of

[2] This study was made in collaboration with Dr. Helen Thompson, Research Associate in The Yale Psycho-Clinic, and is reported in detail in a forthcoming monograph [Learning and growth in identical infant twins: an experimental study by the method of co-twin control, *Genet. Psychol. Monog.*, July, 1929, Volume VI].

behavior, stair climbing and cube behavior, including prehension, manipulation and constructive play with a dozen one-inch red cubes. An experimental staircase arrangement of 5 treads was used, and for 10 minutes daily Twin T was put through her paces. At 48 weeks she scaled the stairs for the first time with slight assistance. At the conclusion of the 6 weeks training period (age one year) she was a relatively expert climber. At that age her untrained Co-twin C, would not yet scale the staircase, even with assistance. At the age of 53 weeks however, when C was again confronted with the staircase she climbed to the top without any assistance and without any previous training whatsoever. In this sense the form and the efficiency of her pattern of climbing were almost purely a function of the maturation of the appropriate neural counterparts.

Twin C was then given an experimental course of training in stair climbing, two weeks in length. At the end of this period (age 55 weeks) she approached Twin T in her climbing skill. By means of the motion picture it was possible to make a comparison of the climbing ability of C at 55 weeks (after 2 weeks of training) with that of T at 52 weeks (after 6 weeks of training). This comparison introduced an interesting form of relativity into the investigation and brought out the significant fact that although T had been trained three times longer and seven weeks earlier, this advantage was more than overcome by the three weeks of C's added age. Again the powerful influence of maturation on infant behavior pattern is made clear. Early training altered slightly the form of the pattern, and hastened the acquisition of facility, but left no considerable or decisive locomotor advantage in favor of Twin T.

In the field of cube play the experiment clearly showed that training had no significant effects upon the patterns of prehension, manipulation and constructive exploitation. Although Twin C had enjoyed no special opportunities in the handling of cubes, her cube behavior was fully equal to that of T after a 6 weeks training period. The similarity in temporal and spatial details of pattern was confirmed in this

case by a time-space analysis of the behavior patterns by means of the cinema record. This does not mean, however, that there were no changes in the patterns of cube behavior during the training period from 46 weeks to 52 weeks. On the contrary, the records, when analyzed, show consistent and incontrovertible weekly increments. Indeed, a day by day analysis of the diurnal records of cube behavior satisfied us that there was a daily drift toward progressive changes in the cube performance patterns. These changes were developmentally achieved by steady processes of decrement and increment rather than by a saltatory or zigzag course. There may be spurts and plateaus and rhythms in the development of other fields of behavior, but at this stage of the life cycle there was a relatively constant trend toward daily change. This progressive daily changing apparently occurs by a process of continuous emergence which tends to lift the level of development slowly and steadily as though by tide action rather than by rhythmic spurt. We would explain the resistance of the patterns of cube behavior to the influences of training and conditioning by the fact that these patterns are basically under the stress and the regulation of the intrinsic organic factors of maturation. The very fact that there is a growth trend toward daily change of pattern makes the behavior less susceptible to stereotypy and to conditioning.

4. *The Restricted Influence of Physical Handicap.*—This subject opens up the vast field of experimental etiology in which the conditions of disease and environmental abnormality may be analyzed to determine the influence of extrinsic factors upon the complex of growth. In many instances these extrinsic factors seem to be much less powerful than one might suppose. Even grave degrees of malnutrition, correlated with excessive subnormality of weight, are usually incompetent to inflict any drastic changes upon the forms of fundamental behavior patterns and upon the genetic order of their sequence. While it must be granted that certain food deficiencies, for example in the field of calcium metabolism, may definitely influence the general picture of behavior,

the nervous system itself is remarkably resistant to general adversity, even to malnutrition. When certain areas of the nervous system are actually damaged by disease or injury, maturation cannot make amends, but the maturation of the nervous system seems to proceed toward the optimum in the areas unimpaired, even though lacking the stimulus of exercise of the functions controlled by the impaired areas. It is for this reason that certain clinical types of profound motor disability attain none the less considerable approximation to normality in certain patterns of behavior.

In this context we may also mention the high degree of autonomy which the nervous system maintains even in extreme cases of *puberty præcox*. We have investigated one case in which there was a precocious displacement of puberty amounting to a whole decade. This girl became physiologically mature at the age of $3\frac{1}{2}$ years. In spite of this extreme developmental alteration, the course of her behavior development in the fields of intelligence, language and locomotion has been relatively normal and stable.

Here, also, should be mentioned the general developmental course of the healthy infant born after an abnormally short or an abnormally long gestation period. A premature postnatal environment and a protracted uterine environment must be considered as drastic deviations from normal environmental influence. The relative immunity of the behavior patterns from these environmental deviations again bespeaks the potency of maturational factors.

5. *Developmental Progression in Emotional Behavior.*—The rôle of maturation in the control of emotional behavior has had scant recognition. The primary emotions have been discussed as though they were elementary stable phenomena subject only to the changes of social conditioning. This is the implication in much that has been written concerning the emotion of fear. It seems to us that the problem has been over-simplified. Fear may be an original tendency, but it is subject to the genetic alterations of organic growth as well as to organization by environmental conditioning. Such conditioning may determine the orientation and reference of

fears, but the mode of fearing undergoes change as a result of maturation. Fear is neither more nor less of an abstraction than prehension. It is not a simple entity. It waxes and alters with growth. It is shaped by intrinsic maturation as well as by experience, certainly during the period of infancy.

Consider for example the reactions of an infant to confinement in a small enclosed space, approximately 2 x 3 x 4 feet. In a physical sense the situation is entirely harmless. The space is ample in size, it is ventilated, it is illuminated, it is open at one end. In a personal sense however, the space may have elements of novelty and unusualness. The infant is not accustomed to lie in such a small space which shuts him off from his accustomed environment. What are his reactions, even when he is gently introduced into this enclosed chamber? At 10 weeks he may accept the situation with complete complaisance; at 20 weeks he may betray a mild intolerance, a dissatisfaction, persistent head turning and social seeking which we may safely characterize as mild apprehension; at 30 weeks his intolerance to the same situation may be so vigorously expressed by crying that we describe the reaction as fear or fright. Here then are three gradations of response: first, no disquietude; second, mild disquietude; third, robust disquietude. Is not this a genetic gradation of fear behavior which is based upon maturational sequence rather than upon an historical sequence of extrinsic conditioning factors? Such factors may account for specific aspects of fear behavior, but not for the organic pattern beneath such behavior. This pattern, we would suggest, is as much the product of organic growth as the various stages in the elaboration and perfection of prehension. Incidentally it may be said that the observation of duplicate twins will tend to substantiate the existence of maturational factors in the development of emotion. Although the tendency towards developmental divergence in identical twins is probably greater in the field of personality make-up than in any other sphere of behavior, there is, during infancy, an impressive tendency toward identity of emotional behavior. Twins T and C, already referred to, showed a highly significant

degree of correspondence in their manifestations of initial timidity, in their responsiveness to social games, in their reactions to the mirror image, in their gestures of avoiding and refusing, in their seeking and begging gestures, in their laughter and crying. The relatively simultaneous and progressive nature of these changes in the field of emotional behavior suggests the influence of organic maturational factors as opposed to purely extrinsic factors in the determination of behavior pattern.

The extreme versions of environmentalist and conditioning theories suffer because they explain too much. They suggest that the individual is fabricated out of the conditioning patterns. They do not give due recognition to the inner checks which set metes and bounds to the area of conditioning and which happily prevent abnormal and grotesque consequences which the theories themselves would make too easily possible. Although it is artificial to press unduly a distinction between intrinsic and extrinsic factors, it must after all, be granted that growth is a function of the organism rather than of the environment as such. The environment furnishes the foil and the milieu for the manifestations of development, but these manifestations come from inner compulsion and are primarily organized by inherent inner mechanics and by an intrinsic physiology of development. The very plasticity of growth requires that there be limiting and regulatory mechanisms. Growth is a process so intricate and so sensitive that there must be powerful stabilizing factors, intrinsic rather than extrinsic, which preserve the balance of the total pattern and the direction of the growth trend. Maturation is, in a sense, a name for this regulatory mechanism. Just because we do not grant complete dichotomy of internal and external factors, it is necessary to explain what keeps the almost infinite fortuities of physical and social environment from dominating the organism of the developing individual.

The organismal concept requires that the individual shall maintain an optimum or normal integrity. The phenomena of maturation suggest the stabilizing and inexpugnable factors

which safeguard the basic patterns of growth. Just as the respiration of the organism depends upon the maintenance of constant hydrogen-ion concentration, so probably on a vastly more intricate scale, the life career of the individual is maintained by the physiological processes of maturation—processes which determine in such large measure the form and the sequence of infant behavior pattern, that the infant as an individual is reasonably secure against extreme conditioning, whether favorable or unfavorable.

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CHANGING TENDENCIES IN GENERAL PSYCHOLOGY

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Although psychology has been changing its scope and method significantly for a number of years, the extent to which it differs from the psychology of a quarter of a century ago has been difficult to determine. The well-worn statement that psychology first lost its soul, and then its consciousness, appears to be sound according to objective analysis of textbooks.

This paper is an attempt to examine the content of a number of representative texts in general psychology published over a period of thirty-nine years, and by such examination to note some general tendencies in psychological theory and practice. The investigation involves the analysis of 110 texts in general psychology listed according to year periods as follows: 1890–99, 27 texts; 1900–09, 21 texts; 1910–19, 23 texts; and 1920–28, 39 texts. In this connection two terms should be carefully noted, 'general' and 'text.' The term 'general' as here understood, means that the books examined were designed to present a general treatment of psychological phenomena—not special treatises on certain aspects of psychology. The second term, 'texts' is important because it was thought that textbooks would come nearest to depicting the content of the general field for any period.

It was found that the content of general psychology could be classified under twenty-seven headings and topics.[1] It should be stated, however, that many of these headings overlap, and probably no classification would be entirely satisfactory to all concerned. The method employed was that of noting the number of chapters and the number of

[1] The writers were aided by the classification of Robinson and Robinson, Readings in general psychology.

TABLE I

DISTRIBUTION OF GENERAL PSYCHOLOGY SUBJECT MATTER ACCORDING TO DECENNIAL PERIODS

Subject Matter Topics	1890–1899				1900–1909				1910–1919				1920–1928				Chapters Total		Pages Total	
	Chapters		Pages		Chapters		Pages		Chapters		Pages		Chapters		Pages					
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
1. Nature, problems, and methods of psychology	33	7.8	383	4.4	32	11.0	411	9.3	29	7.0	536	8.2	41	5.6	698	5.2	135	7.37	2,028	6.19
2. Structure and function of nervous system	20	4.7	449	5.2	20	6.9	565	13.0	17	4.2	308	4.5	42	5.7	853	6.4	99	5.40	2,175	6.64
3. Reflex action and instinct	19	4.4	443	5.2	12	4.1	222	5.0	29	7.0	519	7.9	77	10.7	1,333	10.2	137	7.48	2,517	7.69
4. Habit and learning	7	1.6	241	2.8	8	2.7	131	3.2	24	6.0	465	7.1	61	8.4	1,076	8.0	100	5.46	1,913	5.84
5. General facts about sensation, consciousness and soul	73	17.0	1,435	16.0	42	14.6	727	16.8	71	17.7	1,186	18.4	48	6.6	808	6.1	234	12.78	4,156	12.69
6. Nature and functions of sense organs	21	4.9	389	4.5	12	4.1	127	2.9	7	1.7	45	.7	16	2.2	299	2.2	56	3.06	860	2.62
7. Attention and interest	21	4.9	362	4.2	19	6.5	321	7.3	17	4.2	217	3.3	32	4.4	653	4.1	89	4.86	1,553	4.74
8. Perception and ideas	36	8.5	806	9.4	35	12.0	427	9.9	32	8.0	590	9.1	44	6.2	1,012	7.6	147	8.03	2,835	8.66
9. Imagination and imagery	20	4.7	460	5.4	11	3.8	129	3.7	17	4.2	128	1.9	29	4.0	454	3.4	77	4.20	1,171	3.58
10. Association and memory	32	7.5	720	8.4	17	5.8	241	5.4	40	10.0	547	8.3	47	6.5	840	6.3	136	7.43	2,348	7.17
11. Language	3	.7	69	.8	2	.6	24	.5	5	1.2	62	.9	8	1.1	237	1.8	18	.98	392	1.19
12. Judgment, reasoning, suggestion, thinking, etc.	49	11.0	879	10.3	24	8.2	256	5.8	22	5.5	399	5.9	33	4.5	664	5.7	128	6.99	2,198	6.71
13. Emotion and feeling	50	11.0	995	11.7	21	6.9	316	7.2	23	5.6	428	6.7	46	6.3	746	5.6	140	7.65	2,485	7.69
14. Personality, character, and temperament, etc.									8	2.0	144	2.2	17	2.3	388	2.9	25	11.36	532	1.62
15. Individual differences, intelligence, measurement and statistics					5	1.6	36	.8	10	2.5	219	3.3	37	5.1	672	5.0	52	2.84	927	2.83

TABLE I—*Continued*

Subject Matter Topics	1890–1899				1900–1909				1910–1919				1920–1928				Chapters Total		Pages Total	
	Chapters		Pages		Chapters		Pages		Chapters		Pages		Chapters		Pages					
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
16. Work, effort, fatigue, rest, etc.					4	1.2	32	.7	4	1.0	45	.7	11	1.5	124	.9	19	1.03	201	.61
17. Will, volition, and character	31	7.4	746	8.7	7	2.4	89	2.0	10	2.5	217	3.2	28	3.8	466	3.5	76	4.15	1,518	4.63
18. Dreaming, psychoanalysis, hypnotism, telepathy, spiritualism	2	.4	30	.3					3	.7	42	.6	17	2.3	382	2.1	22	1.20	454	1.38
19. Abnormal types of behavior	6	1.4	113	1.3	4	1.2	53	1.2	5	1.2	53	.8	17	2.3	350	2.6	32	1.74	569	1.73
20. Relation between physical and mental ability					6	2.0	128	2.8	4	1.0	52	.7	2	.3	68	.5	12	.65	248	.75
21. Application of psychology to business, industry, school subjects, etc.					6	2.0	85	1.9	5	1.2	64	.9	26	3.5	534	4.7	37	2.02	683	2.08
22. Laws and methods of heredity					3	1.0	63	1.4	4	1.0	67	1.0	12	1.6	179	1.4	19	1.03	309	.94
23. Moral, social, religious behavior and consciousness									9	2.2	150	2.1	18	2.4	309	2.3	27	1.47	459	1.40
24. Influence of continuous work, external conditions, drugs, etc. upon efficiency									3	.7	30	.4	1	.1	9	0.0	4	.21	39	.11
25. Mental hygiene									1	.2	29	.4	2	.3	29	.2	3	.16	58	.17
26. Exceptional children (subnormal and supernormal)													3	.3	55	.4	3	.16	55	.16
27. Vocational guidance													3	.3	50	.3	3	.16	50	.15
Totals	423	97.9	8,520	98.6	290	98.6	4,383	100.4	399	98.5	6,542	100.2	718	98.9	13,288	99.4	1,830	99.87	32,733	99.87

pages devoted to the various topics. In a few instances authors did not divide their books into chapters but topics, and for our purposes these were all classified as chapters.

Results

Table I gives the distribution of general psychology subject matter according to year periods. Here the topics are listed according to the number and percentages of chapters and pages allotted to each. Since the number of books examined varied with the year periods, the important facts are found in the percentage columns, which are computed by periods for chapters and pages respectively. One of the most conspicuous features of Table I is the observation that psychology has markedly increased its scope during the past two periods. This is manifested especially by the increase of topics from 1910 to 1928. Attention is directed to a few points of interest with respect to the topics themselves. Under Topic I (the nature, problems and methods of psychology) were classified all those subjects which might be considered introductory to the books as a whole. The results indicate that a relatively great amount of space was given to this topic during the first two periods, and a gradual decrease in the third and fourth. This condition is probably explained by the expansion of the scope of the field of psychology. This topic was rivaled by others, thus making it imperative that there be a decrease in the number of chapters and pages allotted.

Topic 2 (structure and function of the nervous system) maintains a constantly prominent position throughout the entire four periods. This topic is usually discussed early in all texts and is certainly basic to all later discussion. During the first two periods (1890 and 1900) there was a tendency to emphasize structure at the expense of function. During the last two periods discussion of the function of the nervous system was constantly held in the foreground. In other words, about this time there was a tendency for psychologists to gradually minimize the structural and to emphasize the functional psychology.

Topic 3 (reflex action and instinct) indicates an increase in per cent with each additional period. This tendency is contrary to what might be expected from common observation. There was a question as to the advisability of

FIG. 1. A comparison of the various topics by decennial periods in terms of percentage of chapters.

classifying reflex action with instinct, since in recent years reflex action has to a considerable extent supplanted instinct. The recent practice has been to outline the basic factors in the behavior mechanism, and to show the significance of reflex action to the learning process. The plasticity of the nervous system with a consideration of the properties of the neurones has been foremost in the discussion of recent years. During the first two periods, the works of the early writers were concerned with the classification of instincts and their importance as drives and forces in the individual, without much reference to the simpler reflex mechanisms. The work of Watson and his followers during the last two periods has no doubt influenced the discussions of instincts, although the majority of writers have not whole-heartedly accepted this point of view. The discussion of habit and learning (4) as in the case of reflex action and instinct shows an increase of late years in percentage of space. It was to be expected that the importance of habit and learning would be accentuated with the anticipated decline of reflex action and instinct. However, those who emphasize reflex action (especially in recent years) are also inclined to emphasize habit and learning —particularly learning. If the individual is born with a large number of reflexes, but with few mechanisms of behavior which may be called instinct proper, then it is only natural that learning will be emphasized. Thus, original and acquired behavior should show increases in importance together.

Topic 5 (general facts about sensation, consciousness, soul, etc.) has the greatest percentage of space during the first three periods, and only a relatively important rank in the fourth. The facts as presented show a condition which would actually not be indicated had a separate classification been made for consciousness and the soul. The 'soul' as a psychological entity was of considerable importance during the early periods, but lost its prominence almost entirely with the beginning of the twentieth century. Consciousness apparently having its initiation with the termination of the use of the term 'soul,' held a dominant position until the

beginning of 1920, but has been gradually giving way to the influence of the concept 'behavior' during recent years. According to the results presented, it is possible that in the next decade consciousness as an element in psychology will also become extinct in psychological literature. The reason for this is due to the development of objective psychology and its concomitant methods of approach to problems. Consciousness has no practical utility in the psychological laboratory, however important it may be in the interpretation of results where objective scientific experimentation is limited.

Other topics frequently discussed during the early periods include 12 (judgment, reasoning, suggestion, and thinking). This general topic often appears in the books of the first two periods, with marked decline in the last two. The influence of the intelligence testing movement and its attempt to measure and correlate the higher mental functions has probably been influential in effecting the decrease in significance of these subjects. The theory of 'water tight compartments' and its derivative, the 'dogma of formal discipline' is evidenced clearly in the first period with some effect during the second. During the third and fourth periods, many authors apparently apologize directly or indirectly for their tendency to discuss the various higher mental functions separately, feeling perhaps that in doing so they may be understood as leaning toward the exploded theory of formal discipline. This tendency to discuss the so-called mental functions individually, prevails even today with little possibility of any marked departure from the psychological traditions now more than a quarter of a century old.

Topic 13 (feeling and emotion) occurs most frequently during the first period with a lessening influence throughout the others. This is consistent with the waning significance of sensation and consciousness for the same periods. This topic is often discussed by modern writers with instinct and reflex, and is thus classified with original behavior equipment. Among the early writers, it was closely allied with consciousness and allied psychological elements. As would be expected, will, volition, and character (17) have most frequent

mention during the first period, but still remain in a significant position during the fourth period. The significance of the topic during the fourth period may be partially due to the fact that character as a personality trait has come into new significance in recent years as a result of the measurement movement, and the treatment of character as a personality trait. The tendency among modern writers is to explain character and the emotional traits of the individual in terms of habit and nerve action. This viewpoint is in contradistinction to the older writers, particularly James and his followers who, in attempting to explain will, volition, and character, used the psychological rather than the neurological approach.

The other topics listed in Table I stand out in varying degrees of prominence according to the periods in which they are listed. As will be noted, many topics do not occur during the first and second periods, and some do not occur until the fourth. The attempt of psychologists to apply psychological principles to business, industry, and school subjects is evidenced in the second, third, and fourth periods. These facts substantiate the belief that psychology is losing some of its 'pure' characteristics and extending itself into the field of applied science. The inclination of recent authors to make applications of psychological principles is no doubt useful in developing interest on the part of students, and creating a demand for general psychology courses. It is well known that in some institutions officials in psychology departments have found themselves in critical circumstances with regard to student patronage because of the competition of education departments which are constantly striving to incorporate educational psychology and allied fields.

Topics Arranged in Order of Importance by Decennial Periods

In order to throw further light on the changing tendencies of psychology, the topics of Table I have been arranged in the order of their importance, as determined by the percentage of the total chapters for each devoted to the various topics. Since it was found that the relation between the percentage

of chapters and the percentage of pages was very close, the ranking below is based on chapters alone. Those topics most important according to the percentage of space allotted come first, the next highest second, and so on through all the topics.

Topics Ranked in Order of Importance

1890–99 (inclusive)

No. of Topic	Order
5	1. General facts about sensation, consciousness, and the soul
13	[2][2]. Emotion and feeling
12	3. Judgment, reasoning, suggestion, thinking, etc.
8	4. Perception and ideas
1	5. Nature, problems, and methods of psychology
10	6. Association and memory
17	7. Will, volition, and character
6	8. Nature and functions of sense organs
7	9. Attention and interest
2	10. Structure and function of nervous system
9	11. Imagination and imagery
3	12. Reflex action and instinct
4	13. Habit and learning
19	14. Abnormal types of behavior
11	15. Language
18	16. Dreaming, psychoanalysis, hypnotism, telepathy, spiritualism, etc.

1900–09 (inclusive)

5	1. General facts about sensation, consciousness and soul
8	2. Perception and ideas
1	3. Nature, problems, and methods of psychology
12	4. Judgment, reasoning, suggestion, thinking, etc.
13	5. Emotion and feeling
2	6. Structure and function of nervous system
8	7. Attention and interest
10	8. Association and memory

[2] Topics found in brackets are of equal importance according to percentage of space.

6 ..... 9. Nature and functions of sense organs
3 ..... 10. Reflex action and instinct
9 ..... 11. Imagination and imagery
4 ..... 12. Habit and learning
17 ..... 13. Will, volition, and character
20 ..... 14. Relation between physical and mental ability
21 ..... 15. Application of psychology to business, industry, school subjects, etc.
15 ..... 16. Individual differences, intelligence, measurement, and statistics
16 ..... 17. Work, effort, fatigue, rest, etc.
19 ..... 18. Abnormal types of behavior
22 ..... 19. Laws and methods of heredity
11 ..... 20. Language

1910–19 (inclusive)

5 ..... 1. General facts about sensation, consciousness and soul.
10 ..... 2. Association and memory
8 ..... 3. Perception and ideas
1 ..... 4. Nature, problems, and methods of psychology
3 ..... 5. Reflex action and instinct
4 ..... 6. Habit and learning
13 ..... 7. Emotion and feeling
12 ..... 8. Judgment, reasoning, suggestion, thinking, etc.
2 ..... 9. Structure and function of nervous system
7 ..... 10. Attention and interest
9 ..... 11. Imagination and imagery
15 ..... 12. Individual differences, intelligence, measurement, and statistics
17 ..... 13. Will, volition, and character
23 ..... 14. Moral, social, religious behavior and consciousness
14 ..... 15. Personality, character and temperament
6 ..... 16. Nature and function of sense organs
11 ..... 17. Language
21 ..... 18. Application of psychology to business, industry, school subjects, etc.
19 ..... 19. Abnormal types of behavior
16 ..... 20. Work, effort, fatigue, rest, etc.
22 ..... 21. Laws and methods of heredity
20 ..... 22. Relation between physical and mental ability

18..... 23. Dreaming, psychoanalysis, hypnotism, telepathy, spiritualism
24..... 24. Influence of continuous work, drugs, and external conditions upon efficiency
25.....25. Mental hygiene

1920–08 (inclusive)

3..... 1. Reflex action and instinct
4..... 2. Habit and learning
5..... 3. General facts about sensation, consciousness, etc.
10..... 4. Association and memory
13..... 5. Emotion and feeling
8..... 6. Perception and ideas
2..... 7. Structure and function of nervous system
1..... 8. Nature, problems and methods of psychology
15..... 9. Individual differences, intelligence, measurement, and statistics
12.....10. Judgment, reasoning, suggestion, thinking, etc.
7.....11. Attention and interest
9.....12. Imagination and imagery
17.....13. Will, volition, and character
21.....14. Application of psychology to business, industry, school subjects, etc.
23.....15. Moral, social, religious behavior and consciousness
19..... 16. Abnormal types of behavior
18..... 17. Dreaming, psychoanalysis, hypnotism, telepathy, and spiritualism
14..... 18. Personality, character, and temperament
6.....19. Nature and functions of sense organs
22.....20. Laws and methods of heredity
16.....21. Work, effort, fatigue, rest, etc.
11.....22. Language
20..... 23. Relation between physical and mental ability
25..... 24. Mental hygiene
26..... 25. Exceptional children—subnormal and supernormal
27..... 26. Vocational guidance
24.....27. Influence of continuous work, drugs, and external conditions upon efficiency.

General Conclusions

The general purpose of this paper was to analyze representative texts in general psychology with a view to studying trends and determining lines along which psychology is now being formulated. A few general implications may be outlined.

1. The scope of general psychology [as indicated by the expansion of topics] has shown a gradual development during the past thirty-nine years. The most marked expansion has come during the later periods.

2. It is clearly evident that general psychology has not divested itself of many vestigial traditions. The essential changes which have taken place have been largely due to the addition of new topics and the incorporation of experimental data, rather than improvement in the presentation of traditional topics. The tendency of even modern psychologists is to adhere to the treatment of the higher mental functions as individual entities, rather than to a discussion of the interrelation and interdependence of the mental functions as a whole.

3. Psychological associations could make a distinctive contribution to their field by standardizing terminology, and formulating the objectives and aims of general psychology courses. Satisfactory classification and outline of topics is impossible until more definite objectives are determined.

4. The content of general psychology may well devote more space to the fundamental elements of behavior, including the structure and function of the nervous system in its direct relationship to learning and acquired reactions.

5. Present day authors of psychology, though trained in the old school, could improve their treatises to an appreciable extent by evaluation of some of the important principles of behaviorism. As a method of study in psychology, behaviorism probably lends itself more readily to scientific experimentation than any previous method.

[MS. received February 4, 1929]

THE PROCEDURE OF THINKING ABOUT MIND[1]

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This paper does not pretend to give a final answer to the question 'what is mind?' but rather to suggest a procedure for approaching questions about the description and explanation of mental activities. The writer's best hope is to assist in clarifying the notions about mind by stating them in different language, and by suggesting a way to think about mind and body and about causal, genetic and other explanations of individual behavior which seems to remove some of the conflicts among systems of psychology.

The solution of problems is often made easier by first stating them negatively. When one says, 'How should I not think about mind?' the points of agreement between him and others become emphasized. The recent psychologies seem to agree that it is not good thinking to treat consciousness, subconsciousness, or the mind as if they were entities which produce certain observable facts.

Here psychology is in agreement with physics which discards a conception of force that treats force as an unobservable thing influencing its manifestations. Gravity, for example, is not to be thought of scientifically as an unobservable thing which makes objects fall to the ground. For science, the force of gravity refers to the group of observable facts that bodies approach each other relatively to their masses and inversely as to the squares of their distances. In the same way 'memory' in psychology is not the thing which makes us remember, but it is a group term to use when referring to the observable facts of memory and to their observable relationships. There is general agreement that such terms as mind are properly used for classifying facts which show certain characteristics and relationships. The novice is to

[1] Read before the Southern Society for Philosophy and Psychology.

be cautioned against the temptation into which the forms of language often entrap him. It is so easy to use a word like 'mind' as if it represented a ghostlike object back of the observed experiences. Such 'ghost theories,' as Meyer has dubbed them, are found useless by science for understanding or controlling behavior. As group terms to signify observed facts and events, 'conscious' and probably 'subconscious' and 'mental' are still needed for the purpose of classifying facts.

A few behaviorists would add another negative statement about mind. The radical behaviorist would say with Watson,[2] "The time has come when psychology must discard all reference to consciousness." The danger of speculative fancies and the disagreement among some introspective records are not adequate reasons for rejecting all conscious facts from the consideration of science. Observations must be verified in all sciences, but the range of error among observations may or may not be great enough to affect conclusions in physics or in psychology. The usefulness of the descriptions of conscious facts is shown by the awkwardness of the descriptions which the radical behaviorist tries to substitute for them.

Psychology, like other sciences, must define its field and describe its subject-matter; but, if the science stopped there, it would be of little consequence. This paper is not concerned primarily with the definition of what are psychological facts. It accepts as the field of psychology the study of both conscious and bodily activities of individuals. The most important procedure in thinking about such individual activities has to do with the explanation of such activities for the purpose of controlling them. The remainder of the paper, therefore, will be devoted to a suggestion toward harmonizing various scientific explanations of individual activities. To many psychologists causal, genetic, purposive and gestalt explanations, not to mention others, appear to be in direct conflict. The suggestion for a procedure in thinking about explanations seems, therefore, to be especially

[2] J. B. Watson, Psychology as the behaviorist views it, PSYCHOL. REV., 1913, 20, 163.

necessary at this time. By explanation is here meant a relationship among facts other than that used for defining, naming or classifying the facts.

Mental activities, that is, the relations found among conscious events, are unique in some sense. As Driesch and McDougall [3] have pointed out, there is not a point to point identity between qualitative changes in the manifold of conscious events and the quantitative changes of matter in motion. The change in consciousness from red to yellow can be introspectively described in terms of similarity to the two limiting and qualitatively different facts, red and yellow. For the mechanist, the change from red to yellow is only a quantitative difference in length of wave. Yellow is only a shorter wave. No analogy, therefore, for understanding the change in consciousness from red through orange to yellow, which does not account for the qualitative difference between red and yellow, can be wholly adequate. This qualitative change a mere difference in wave-length fails to explain. The physical series must be supplemented by some sort of predictable conscious series, perhaps of steps of resemblance to red.

In searching for helpful analogies to understand psychophysical behavior, one seems to be forced to discard as seriously incomplete, all analogies for conscious and bodily activities which are statable only in mechanical terms. If this position is correct, many of the analogies which have been important in the history of thought will be found inadequate. This criticism applies to all analogies which assume that mind and body are identical relationships. Warren, McDougall and others have repeatedly pointed out that such analogies as the concavity and convexity of a shield fail to represent mind and body because they consider conscious and physical facts as if both were perceived in the same way. As McDougall says, they are really identity hypotheses.

To escape this difficulty, Warren [4] utilized another physical

[3] H. Driesch, Science and philosophy of the organism; W. McDougall, Body and mind, New York, Macmillan, 1911.

[4] H. C. Warren, The mental and the physical, PSYCHOL. REV., 1914, 21, 79–100.

analogy which better represents the psychophysical relationship, but which still retains a physical bias. He suggested that the relationship might be thought of as like that between surface and mass, mass corresponding to physical facts. Discussing this analogy, Warren says, "The surface of a body and its mass or weight are perceived by two distinct senses. . . . Divide a lump of earth or metal into as many parts as you will, your eye perceives only the outer surface of the parts—you never see the mass within. Heft it, and you feel only the weight—the surface is never a datum of muscular perception. Alter the shape as you will, the relations of the constituent particles are altered pari passu with the change of surface relations. Mass and surface change conjointly; they are inseparably bound together; they are two radically distinct aspects of the same thing."

This analogy serves, as he says, "to picture the relation between nervous system and consciousness without supposing them to be two distinct things, either interacting on one another or separate and parallel. The analogy is doubtless imperfect. Mass does not correspond, character by character, to the mental aspect or inwardness of the world; surface does not correspond to the physical aspect or outwardness of the world."

It should be noted in connection with this analogy that, while surface and weight vary conjointly, there may be considerable independence in their variations. The surface of a body may even decrease when its weight increases and vice versa. It is probably just as important for psychology to discover these apparent differences in the variations of conscious and brain facts as to discover the uniformities in these relations.

After the scientist recognizes that for him mind and matter are certain characteristics and relationships found among observed facts, he seems often to conclude that either conscious or physical activity must be the one ultimate characteristic of the universe. If he favors the interpretation of the radical idealist, he says, 'All facts must be conscious in the last analysis because they depend upon introspection.'

He then adopts the elaborate dialectic of the idealist to deduce the result that matter is mind, which common sense vetoes. If he favors the interpretation of the radical materialist, he says, 'Matter and motion are ultimately the only useful characteristics of reality, so consciousness may be neglected,' and he meets the same veto by common sense. The mechanist further objects when the biologist introduces a new type of genetic explanation through growth and evolution which is not reducible to cause and effect.

One escape from such dilemmas in thinking about mind and body, seems gradually to be implicitly recognized by a larger circle of thinkers in science and to meet with philosophical approval. When an event is seen to be causally related in one series of facts, it may be found to be also genetically or purposively related in other series of facts. The discovery that red is causally related to brain, nerve and receptor activities and then to light waves does not prevent the discovery that it is genetically related to yellow and that it becomes a conscious experience only at a later stage of evolution than does yellow. Such stages in evolution are now well established as to color vision. Color-blindness is recognized as an arrested stage of development.

Growing out of interpretations suggested by recent realistic philosophies, another type of analogy than the one proposed by Warren brings out more helpfully this procedure of scientifically tracing the various relationships of a fact. It is an analogy which escapes the physical assumptions of mass and surface. Moreover it is an analogy which may be rather clearly presented to students. No teacher of psychology is able to escape successfully the demand of students for some procedure to aid them in thinking about conscious and bodily relationships. The analogy which the writer has found useful has the advantage that it does not commit the teacher to any of the current hypotheses of materialism, idealism, interactionism, parallelism, teleology or vitalism.

Suppose that instead of mass and surface one thinks of an analogy in which the symbols are not thought of as physical. Such an analogy is furnished by the relationship of letters in

words. One may think of significant neural relationships as represented by the word B–R–A–I–N, which then stands for the actual relations discovered by neurologists. In the same way one may think of the word M–I–N–D as standing for the relations found among conscious facts. The same fact, for example that represented by the letter 'I,' can then be conceived as traced in its brain and its mind relations without contradiction in thought.

The recent discussions of the realists and their theory of knowledge contribute much to the serviceableness of this new type of analogy. The analogy of letter-word relations assumes that the same fact may be studied in different relations, each adding to our understanding of the events and affording a different way of describing, explaining, predicting or controlling that event. The possibility of a fact being related to a conscious and to a physical series at the same time thus becomes no more puzzling than the relationships of the letter 'I' to two significant series of letters. One may diagrammatically represent the analogy by words intercepting at the letter 'I,' thus:

M
B–R–A–I–N
N
D

Somewhere in each series one arrives at the same event 'I,' and each series thus throws new light upon the fact represented by the letter.

This way of thinking about the interrelationship of physical relations and conscious relations, provides part at least of the advantages claimed for Warren's analogy of mass and surface. It permits an escape from the old discussion between parallelism and interaction. It makes it possible to recognize that neither hypothesis is necessary. Moreover it avoids the mechanical bias of a physical analogy. Under the letter-relationship analogy it becomes possible to understand how a fact in its various relations may be scientifically studied without committing one's self to any exclusive ultimate hypothesis. The letter-series analogy is

much easier to grasp than the more complicated relations of mathematical symbols, useful as the latter may be for other purposes.[5]

When thinking about the interpretation of observed relations in science, this analogy enlarges one's horizon. Not only mind and matter, but various types of explanation may be considered from its viewpoint. Continuing with the analogy, a fact may be considered as occurring in 'mechanical,' 'genetic,' and 'purposive' relations, and possibly those claimed by the 'gestaltist' and the 'vitalist.' In each of these the fact would be represented by the letter 'i' which occurs in each of these symbolic series of letters. The different relationships might be considered to extend from the letter 'i' in as many different ways. The use of different explanatory series would no longer be a question of whether they were logically consistent. They would cease to be necessarily contradictory. Mechanism would lose its copyright on explanation. The value of other explanations would become a question of whether they were useful in predicting or controlling events. They would stand or fall on criteria of usefulness, not of logical contradiction.

The letter-series analogy thus allows for different explanations bearing upon the same fact. For example, it is recognized that one could understand, predict and control 'red' better after a series of causal relations was found connecting it with nerve and ether motion. A different method of prediction and control was afforded when 'red' was found to be in a genetic relation to yellow, arising at a later point in the evolutionary series. Again 'red' may be observed to be connected with various purposeful series, each serving a different end, as when it arouses an attack by a bull, bomb throwing by an anarchist, or oratory by a politician. In the last two cases the purposive series, which includes an appeal to ideas and emotions, is much more fully observable and practically important for controlling the eventual action than the observable mechanical series of stimuli to the eyes

[5] J. M. Fletcher suggests that cross-word puzzles afford the same analogy with serial relations in various directions.

followed by movement. In the prediction of a purposeful series the expectation of the end might well be found to occur with a certain probability, in which case the prediction would be quite as scientific as that of cause and effect. The mental series, even if it remained mechanically inexplicable, would be taken into account in modifying the events.[6]

If breadth of thinking leads to discoveries in psychology or biology, this possibility of freedom from contradiction and a broader consideration of relationships is a desirable accomplishment. When one attempts to interpret an event, every repeating series in which the event occurs should be observed and recorded, regardless of whether such series are causally conceivable, in the sense of being reducible to changes of motion in matter. The analogy thus becomes a safeguard against narrow-mindedness.

This interpretation assumes that every individual organism and special system of thought may have its own unique contribution to make to understanding the universe. By tradition scientists are skittish in regard to concepts which attempt to supplement mechanism. They fear being led off into Elysian realms of pure fancy. But this analogy is an invitation to observe other series, not to construct imaginary series. The concept of series may be more fundamental for controlling complex activities like those of man than the single explanatory concept of a causal series in which all factors must be stated in terms of mechanism.[7]

The search for a monistic universe involves the danger of neglecting facts which do not fit into a system. It is possible

[6] An example of how the letter-series analogy permits a new interpretation of mind to be introduced in thought without contradiction, may be given in connection with 'The realm of mind' by F. J. E. Woodbridge. He believes that a fundamental characteristic found among mental facts is their logical relationship. Woodbridge makes it clear that the discovery of this relationship in no way conflicts with the study of mental facts which is commonly undertaken by the psychologist from the point of view of their other relationships. In his 'Mind as behavior,' Singer states a conception of the various relationships of observed facts which serves also to illustrate the usefulness of this analogy for understanding causal and purposive relations with predictable results.

[7] See the writer's caution against overlooking differences during the constant scientific search for similarities, in his paper 'The significance of qualitative differences for psychology,' PSYCHOL. REV., 1927, 34, 10–27.

that a pluralistic universe is quite as essential for understanding man as a monistic universe. It is even conceivable that the most useful conception of the unity of the universe is not to be found in the picture of a uniform material in motion, but in the single final result of a progression. The search of science along the way may be for partial views of events, in which the relations of cause, genesis, purpose, etc., may aid in foreseeing and controlling the events.

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THE USE OF BIBLIOGRAPHIES IN PSYCHOLOGY

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Initiation, or at least completion, of productive research demands a familarity with the pertinent literature. The reports of previous work of a similar nature are published in many places and in many languages. To search the whole literature volume by volume is obviously impracticable, even impossible. So one turns to the indexes or to previously published bibliographies. The latter, representing as they do, some sort of selection, and dealing with specific topics, are most valuable. If the usefulness of this experimental tool be admitted it is interesting to learn something of its prevalence and its distribution.

The analysis of material collected for, and published in a comprehensive list of bibliographies on special topics in psychology which appeared during the first quarter of the century [1] forms the basis for this paper. A detailed search of 144 periodicals in psychology and related sciences afforded the bulk of the 2134 bibliographies listed. To be included in the list a bibliography has to meet certain requirements, viz., (1) it must have fifty or more references, the majority of which are specific; (2) it must be reasonably complete for the subject under discussion, either by statement of the author or in the judgment of the compiler; or (3) it must be attached to an historical review or summary. In any case of doubt the reference was included. These criteria ensured the inclusion of the most useful bibliographies, but at the same time may have eliminated some which experts in specific fields would have considered valuable. However, the list as it stands includes the important bibliographies published during the period surveyed.

[1] Louttit, C. M., 'A Bibliography of Bibliographies on Psychology, 1900–1927,' *Nat. Res. Coun. Bull.*, 1928, no. 65, pp. 108.

Table 1 shows the distribution of the journals thoroughly searched by subject and country. Five of the total number have been eliminated because they were of a very general nature and included few, if any, articles of interest to psychologists. The fractions indicate journals which are concerned with two subjects, in which case each has been credited with one half.

TABLE 1

DISTRIBUTION OF JOURNALS BY SUBJECT AND COUNTRY

Subject	U. S.	Ger.	Grt. Brit.	France	Ital.	Scan.	Jap.	Austral.	Totals
Psych., Gen.	13.5	14.5	2.5	5.0	3.0	1.5	1.0	0.5	41.5
Psych., Appl.	11.0	5.0	2.0						18.0
Psychiatry	7.0	4.5	1.0						12.5
Neurology	3.0	4.5	1.0						8.5
Physiology	4.5	5.0	3.0	2.0					14.5
Philosophy	2.5	5.5	0.5	2.0				0.5	11.0
Education	3.5	3.0	2.0			0.5			9.0
Biol. and Zoöl.	4.0	3.0	1.0						8.0
Psychoanalysis	2.0	3.0							5.0
Miscellaneous	10.0					1.0			11.0
Totals	61.0	48.0	13.0	9.0	3.0	3.0	1.0	1.0	139.0

The book literature was not studied so systematically. Approximately three thousand Library of Congress cards (which have notations regarding bibliographies) were examined, as well as the catalogue of the Yale University Library and of the Research Information Service of the National Research Council. From these sources only 256 bibliographies were selected as being sufficiently specific and meeting the criteria. References from miscellaneous sources added 191 to the total. These 447 references are omitted in the analysis which follows.

To secure some data as to the size of the included bibliographies, approximately one tenth of the total number were selected in chance order by taking every tenth entry from the alphabetical list. The number of entries in these 214 bibliographies ranged from 3 to 2,150 with an average of 110. Bibliographies with 100 or fewer references comprised 75.6 per cent of the total number. These data give a fair picture of the quantitative nature of the material included.

The 1687 references remaining after eliminating books and those from miscellaneous sources were further analysed to determine the extent of the publication of bibliographies in various countries at different periods. These data are presented in table 2 as absolute number of bibliographies found.

TABLE 2

ACTUAL NUMBER OF BIBLIOGRAPHIES BY COUNTRY AND YEAR

Period	U. S.	Ger.	Grt. Brit.	Fran.	Scan.	Ital.	Jap.
Before 1900	4	4					
1901–1905	32	68	6	22		1	
1906–1910	95	69	11	18		2	
1911–1915	324	111	9	6	4	3	
1916–1920	285	59	7	3	2	1	
1921–1925	190	158	10	2	3		
1926–1927	109	65	4	2			2
Totals	1,039	534	47	53	9	7	2

The number of bibliographies found per volume searched for United States, Germany, Great Britain, and France is shown in table 3. The other countries have been omitted because of the fewness of volumes published in them. A study of this table shows that the countries stand in the order United States, Germany, Great Britain and France in the publication of bibliographies for the quarter century. This order is retained in all of the five-year periods except 1901–1905 and 1921–1925; in the first of which United States and Germany changed places as did also France and Great Britain. In the 1921–1925 period Germany again stands first.

A second fact to be learned is the steady increase in the number of bibliographies published in United States and Germany and the steady decrease in France and Great Britain. The great reductions in the period 1916–1920 in all countries are attributable to the war and to the depression thereafter. United States and Germany have recovered more quickly than either Great Britain or France. The former two countries also stand first in the number of bibliographies published in connection with reviews of literature as distinct

from original articles. In this respect the United States leads, especially in the periods 1911–1915 and 1916–1920 during which time the Psychological Bulletin had the policy of publishing yearly reviews of most of the important problems of psychology. Some of these had very few references but they have all been included in the compilation.

TABLE 3

NUMBER OF BIBLIOGRAPHIES PER VOLUME[2]

Period	U. S.	Ger.	Grt. Brit.	France
Before 1900	.13	.10	0	0
1901–1905	.32	.43	.40	.57
1906–1910	.67	.41	.42	.36
1911–1915	1.99	.53	.35	.12
1916–1920	1.05	.24	.20	.07
1921–1925	.55	.59	.22	.05
1926–1927	.75	.65	.22	.12
Averages	.87	.45	.28	.22

These data offer presumptive evidence of the value of bibliographies in experimental work. Space in scientific journals is too valuable for editors to publish bibliographies unless there is a demand for them. The two countries from which most of the experimental work comes, United States and Germany, are the ones that have consistently stood at the head in number of bibliographies published, that have recovered most quickly from the effects of the war, and that have devoted most space to periodical reviews of the literature.

The picture presented by the tables is sufficiently clear to need no further detailed elucidation. A question arises, however, as to the adequacy of the bibliographies published. There has been no attempt to make a detailed analysis of the material at hand in this respect. If we consider only the bibliographies published in connection with original articles they may roughly be grouped into six classes:

1. Lists of references.

[2] 'The total number of volumes searched for each of these countries was: United States, 1199; Germany, 1181; Great Britain, 167; France, 241.'

2. 'General' bibliographies.
3. Complete or reasonably complete topical bibliographies.
4. Selected bibliographies.
5. Annotated bibliographies.
6. Classified bibliographies.

The first group includes lists of references which the authors have used or found of interest in connection with their work. If they include only well selected and chiefly specific references this type is valuable in a preliminary survey, although it makes no pretense of being complete.

'General' bibliographies are often overgrown lists of references with none of the latters' modesty. In many cases it is obvious that the author could not possibly have consulted all of his references, and in many cases the carelessness with which they are cited proves that he has not seen them. They of course make no show of critical selection, but rest their *raison d'etre* on the fact that they include so much that whoever is interested may choose as he will.

Group three, complete or reasonably complete topical bibliographies, if carefully compiled are extremely valuable. For very specific subjects they probably make reference to everything that has been written in the field. As the topics become larger the probability of reaching completeness becomes smaller, but even under these conditions they afford a very acceptable guide.

The fourth type, selected bibliographies, if done by someone very familiar with the field is perhaps the most valuable of all. Here the aim is, or should be, to include all valuable original contributions, and to eliminate the notes, reviews, and publications of ephemeral interest.

Intelligent and critical annotations added to any bibliography enhance its value. Such annotations need not be abstracts of the article, but should point out its importance, its relation to the field in general, and briefly its approach to the problem.

Classification according to subject increases the usefulness of any bibliography, except possibly complete ones on a very

specific subject, and saves from deserved oblivion the long 'general' type which otherwise is so difficult to use.

This discussion leads to the fundamental question concerning any bibliography—Why compile it at all? The answer must be because of its potential usefulness. This, of course, is not necessarily true of the shorter list of references which is 'a sort of an exhibit A' designed to demonstrate where the author has gone to secure material upon which his work is based or to supply his historical resumé. Surely, if usefulness is not a justification for a bibliography there is none.

Usefulness implies desiderata. The bibliography should include all of the important work that has been published and eliminate the minor and unimportant material or specifically designate it as such. It should be arranged so as to facilitate reference, either in a straight author alphabet, if not too long and general, or classified by subject. Succinct annotations are, of course, helpful. Some indication of the approximate completeness will aid the user in planning further search of the literature.

Above all the citations must be clear and complete. To refer to an article as 'Jones, E. N., JEP, July 1927' is worthless. While this is an extreme example I have encountered many nearly as bad. Every citation should include the name of the author, the full title of the article, the journal, using accepted abbreviations, the year, volume and number of the first and last page. The lack of any one of these introduces the possibility of error in tracing the reference. Omission of the title in any reference is inexcusable; the user has no idea what the citation is about and must waste time turning back to the text to discover in what connection the reference was made. It is unnecessary at present to enter any further into the subject of reference citation, but the interested reader is referred to several letters on the subject published in *Science* a few years ago.[3]

[3] These letters will be found in *Science*, 1925, 62, 419–420; 1926, 63, 68–69, 231–232, 456–457, 596, and 64, 279–280.

SUMMARY

This paper presents evidence of the increasing use of bibliographies in psychology, which increase seems to be correlated with the growth of the experimental and applied phases of the science. Some evaluation of the types of bibliographies found in the study has been attempted, and suggestions as to the most useful manner of presenting a bibliography have been offered.

[MS. received January 14, 1929]

DISCUSSION

ANALYSIS: CHEMICAL OR PSYCHOLOGICAL?

A Comment on Raymond Wheeler's 'The Action Consciousness'

I. With repolished armor Raymond Wheeler fares forth in the January issue of the *British Journal of Psychology*[1] in defence of his sensationalistic doctrine of volition. His present opponent is Dr. Honoria Wells; but his criticism (to drop the chivalric metaphor) differs in no essential fashion from that which, nearly ten years ago he urged against Michotte, Barratt, and Ach. From an extended experimental investigation of "specific choices between already evaluated . . . tastes of very varying hedonic quality"[2] Dr. Wells concludes that choice involves a "consciousness of 'Self-activity' . . . absolutely distinct from the kinæsthetic and organic phenomena which may or may not be present together with it."[3] Wheeler, on the other hand, adheres to the doctrine, based on his original experimental study of choices between pictures and choices between victrola selections,[4] that choice is completely analyzable into kinæsthesis and supplemental imagery. The marked discrepancy, between his conclusion and that of Dr. Wells, Wheeler unhesitatingly attributes to the different mental set of the two groups of subjects. His own observers were, in his opinion, 'set' to analyze completely, in other words they assumed a 'completely analytical attitude'; Dr. Wells's subjects on the other hand assumed only the 'relatively analytical attitude.' Their analysis, he says, "went no farther than consciousness of action and like phenomena for they were looking for such phenomena and were satisfied when they found them."[5] Had they been 'set' to complete analysis, they would, he maintains, have analyzed the consciousness of action into sensation and image.

As evidence of the incompleteness of analysis on the part of the

[1] The action consciousness, *Brit. J. Psychol.*, 1929, **19**, 253–267.

[2] The phenomenology of acts of choice, *Brit. J. Psychol., Monog.*, 1927, **4** (no. 11), 4, 5.

[3] *Ibid.*, p. 147 (9).

[4] An experimental investigation of the process of choosing, *Univ. Oregon Publ.*, 1920, **1**.

[5] The action consciousness, *op. cit.*, pp. 255, 256.

observers in the Wells investigation, Wheeler quotes from their reported introspections such expressions as 'sense of being drawn in certain direction,' 'feeling of aversion,' 'a sort of hitch,' 'felt helpless,' 'visceral feeling,' 'stretching,' 'made up my mind,' 'great effort.' With these reports he contrasts those of his own observers: "No hitches, swellings, helplessness, strains, visceral feelings, efforts, cognitive content, thoughts, feelings of familiarity or what not," he declares, "are left unanalyzed as is the case with Dr. Wells' introspections. With practically nothing analyzed," he concludes, "during the course of the volitional consciousness we should hardly expect an analysis of consciousness of action and consciousness of self." [6]

My initial purpose, in this brief paper, is to question the soundness of Wheeler's conclusion that the introspections of the Wells observers should be discredited on this ground of the incompleteness of their analysis. I do not, to be sure, deny the greater detail, in the case of kinæsthetic and organic sensations, of the introspective reports of Wheeler's observers; and I am free to confess that I wish the Wells subjects had anticipated their critic by carrying further some of their reported analyses. On the other hand, it seems to me fairly open to question whether, in the interests of time-saving, an introspecter is not justified in ending his report at the point where the results of further analysis are clearly indicated.[7] Thus, to instance some of Wheeler's examples of incomplete analysis: 'a sort of hitch' and 'stretching' are plainly instances of kinæsthesis; and there appears to be no need of carrying further the analysis of 'visceral feeling.' In fact, the charge of an 'only relatively analytic attitude' comes with peculiar ill grace from Wheeler, for the examples which he himself brings forward of his observers' introspections certainly show similar failures to carry sensational analysis to the bitter end. When, for example, Wheeler's observer reports 'a setting of my mouth to whistle a long slow note,' he offers no literally complete analysis of the kinæsthetic sensations which occurred and gives no indication at all of the auditory sensations which must have been present; and the phrase 'a series of visual images of Tetrazzini' [8] certainly refers to a complex, not to an

[6] The action consciousness, *op. cit.*, pp. 260, 261.

[7] For Carroll Pratt's comment on the extremist's conception of analytic introspection, "A fine ideal but one which the nature of our psychophysical organism makes impossible of realization," *cf. J. Phil.*, 1924, 21, 229. For Wheeler's rejoinder to Pratt, *cf.* PSYCHOL. REV., 1925, 32, p. 453.

[8] The action consciousness, *op. cit.*, p. 261[2].

elemental, visual experience. I am not criticising these reported observations of the Wheeler observers: on the contrary they seem to me to carry the report of analysis far enough. The points I am stressing are simply these, that Wheeler tacitly admits the propriety of checking the outflow of introspective record when the nature of the introspected experience has clearly been indicated and that he must, therefore, accord the same privilege to the Wells subjects. In other words, Wheeler must yield the right to explain, by the contrast between complete and incomplete analysis, the divergence of his observers' introspective reports from those of the Wells subjects.

II. The preceding paragraphs seem to me to justify the rejection of Wheeler's answers to the question: why do introspecters differ so markedly in their analysis of choice? The question, however, has still to be answered and is to the full as challenging to the one set of introspecters as to the other. As urged by critics of the doctrine that choice includes a distinctive experience of self-activity, the question takes the following form: If such experience really occurs, how account for the position of Wheeler, a serious, highly-trained, and scrupulous introspecter, who holds that the feeling of self-activity is completely analyzable into sensational terms? In an earlier paper I argued boldly that the alleged discrepancy is really non-existent. The reports of the Wheeler subjects' introspection, I contended, when closely studied, indicate the occurrence, at times, of a 'distinctive experience' of self-activity, inattentively realized.[9] The failure on Wheeler's part to recognize these clear intimations of the experience of self-activity I attributed to sensationalistic bias and to a misconception of the nature of self-awareness.[10] It is no part of my present purpose to reiterate this criticism (though, in my opinion, Wheeler has never met it). I want rather to comment on a second explanation of Wheeler's blindness to the evidences of the distinctive consciousness of self-activity contained in introspective reports (those of his own observers included). This explanation I discover in the conception which he has lately brought forward, of analysis as concerned not with constituent parts but

[9] Fact and inference in Raymond Wheeler's doctrine of will and self-activity, PSYCHOL. REV., 1921, 28, pp. 356 *ff.* In this paper and in a later discussion of 'The divergent results of analysis,' I further argued that Wheeler's sensationalistic conception of volition suffers from his inability to 'distinguish one type of volition from another' in terms of different sorts of kinæsthetic and organic sensation-complexes. (*Cf.* PSYCHOL. REV., 1921, 28, p. 367, and 1923, 30, p. 150.)

[10] *Op. cit.*, PSYCHOL. REV., 1921, 28, p. 370[1].

with causal or temporal consequents. Wheeler elucidates the conception by "a chemical analogy. How is it" he asks, "that the chemist obtains sodium and chlorine upon an analysis of table salt? . . . The chlorine and sodium do not as such exist until the salt is destroyed. Then these two substances come into existence. Analysis," in other words, "creates new existences. Similarly," he continues, "I find that consciousness of action imageless and sensationless as it appears to be in its unanalyzed form becomes, or gives way to, sensation and image under the proper analytical attitude." [11] Now between chemical analysis thus described, with stress on its destructive aspect, and traditional psychological analysis, there is obviously a superficial likeness. For it is a commonplace of psychology that analytic observation changes its object, that when, for example, I have analyzed my irritation I am no longer irritated but merely interested in being irritated.[12] More significant, however, than the likeness between the two sorts of analysis is the difference already stated: that when one analyzes descriptively, or psychologically, one attends to the characters of the analyzed complex whereas in chemical analysis one is concerned with its consequents or products.

I am not now chiefly interested in weighing the respective merits of these two types of analysis, yet I may remark in passing that all who deprecate the forcing of psychology into the moulds of the physical sciences and who seek by analysis a clearer knowledge of the nature of experience will hesitate to replace the psychological by the chemical conception of analysis. The point, however, which I wish to make is the following: that the evident difference between these two concepts of analysis serves to explain the discrepancy between our two groups of introspecters, the Wells observers and those of Wheeler. The former hold, in common with most psychologists, the psychological, or descriptive, theory of analysis. In their analysis, accordingly, of the experience of choosing they

[11] The action consciousness, *op. cit.*, pp. 258^3, 259^5, 262^2. Two comments, neither of them essential to my argument seem important enough to find a place in this footnote. The first is the observation that the upholder of the Michotte-Ach-Wells doctrine will wish to guard himself against any suspicion that he regards the complex experience of choice as 'imageless and sensationless.' The second is the reminder that Wheeler himself, though he seems sometimes to imply that all analysis is of the chemical type, yet does not wholly abandon the older descriptive conception of analysis for the chemical. He refers, for instance, to a 'feeling of activity which *consisted of . . . sensory and imaginal processes*' (The action consciousness, *op. cit.*, p. 263^3. Italics mine).

[12] *Cf.* Wheeler, Persistent problems in systematic psychology, II. The psychological datum, PSYCHOL. REV., 1925, 32, p. 259.

are seeking its constituent factors and they find, along with kinæsthetic and imaginal consciousness, something radically different, a distinctive consciousness of self-activity. Wheeler's observers, on the other hand, in their chemical or sequential form of analysis are asking not what choice *is*, but what it 'becomes' or 'gives way to.' Not unnaturally, these introspecters, specially concerned as they are in the study of sensation, find that the consciousness of self-activity (which most of them report) [13] analyzes into, that is, 'gives way' swiftly to a purely sensational and imaginal consciousness. Thus, there emerges a new explanation of the diversity of these introspective reports. *All* the observers, on this hypothesis, experienced distinctive self-activity. Wheeler's observers failed to report the experience and recorded only the less fleeting, more persistent sensation-complexes which followed on it since (apart from any other reasons for overlooking the activity experience) they were seeking after-effects rather than constituents of choice.

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[13] Fact and inference in Raymond Wheeler's doctrine of will, *op. cit.*, pp. 364*ff*.

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American Journal of Psychology—Ithaca, N. Y.; Cornell University. Subscription $6.50. 624 pages ann. Ed. by M. F. Washburn, K. M. Dallenbach, Madison Bentley and E. G. Boring. Quarterly. General and experimental psychology. Founded 1887.

The Pedagogical Seminary and Journal of Genetic Psychology—Worcester, Mass.; Clark University Press. Subscription $7.00. 700 pages ann. Ed. by Carl Murchison. Quarterly. Child behavior, differential and genetic psychology. Founded 1891.

Psychological Review—Princeton, N. J.; Psychological Review Company. Subscription $5.50. 500 pages annually. Bi-monthly. General. Founded 1894. Edited by Howard C. Warren.

Psychological Monographs—Princeton, N. J.; Psychological Review Company. Subscription $6.00 per vol. 500 pp. Founded 1895. Ed. by Shepherd I. Franz. Published without fixed dates, each issue one or more researches.

Psychological Index—Princeton, N. J.; Psychological Review Company. Subscription $4.00. 300–400 pp. Founded 1895. Edited by W. S. Hunter. An annual bibliography of psychological literature.

Journal of Philosophy—New York; 515 W. 116th Street. Subscription $4. 728 pages per volume. Founded 1904. Bi-weekly. Edited by F. J. E. Woodbridge, Wendell T. Bush and H. W. Schneider.

Psychological Bulletin—Princeton, N. J.; Psychological Review Company. Subscription $6.00. 720 pages annually. Psychological literature. Monthly. Founded 1904. Edited by Samuel W. Fernberger.

Training School Bulletin—Vineland, N. J.; The Training School. Subscription $1. 160 pages ann. Ed. by E. R. Johnstone. Founded 1904. Monthly (10 numbers). Psychology and training of defectives.

Archives of Psychology—Columbia University P. O., New York City. Subscription $6. 500 pp. per vol. Founded 1906. Ed. by R. S. Woodworth. Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology and Social Psychology—Eno Hall, Princeton, N. J. Subscription $5. Ed. by Morton Prince. In cooperation with Henry T. Moore. Quarterly. 432 pages ann. Founded 1906. Abnormal and social.

Psychological Clinic—Philadelphia; Psychological Clinic Press. Subscription $3.00. 288 pages. Ed. by Lightner Witmer. Founded 1907. Without fixed dates (9 numbers). Orthogenics, psychology, hygiene.

Psychoanalytic Review—Washington, D. C.; 3617 10th St., N. W. Subscription $6. 500 pages annually. Psychoanalysis. Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.

Journal of Experimental Psychology—Princeton, N. J. Psychological Review Company. 500 pages annually. Experimental. Subscription $6.00. Founded 1916. Bi-monthly. Ed. by Madison Bentley.

Journal of Applied Psychology—Baltimore, Md.; Williams & Wilkins Co. Subscription $4. 400 pages annually. Founded 1917. Quarterly. Edited by James P. Porter and William F. Book.

Journal of Comparative Psychology—Baltimore: Williams & Wilkins Company. Subscription $5. 500 pages annually. Founded 1921. Bi-monthly. Edited by Knight Dunlap and Robert M. Yerkes.

Comparative Psychology Monographs—Baltimore; Williams & Wilkins Co. Subscription $5. 500 pages per volume. Edited by W. S. Hunter. Published without fixed dates, each number a single research. Founded 1922.

Genetic Psychology Monographs—Worcester, Mass.; Clark University Press. Subscription $7.00 per volume. Two volumes per year, 600 pages each. Ed. by Carl Murchison. Monthly. Each number one complete research. Child behavior, differential and genetic psychology. Founded 1925.

Psychological Abstracts—Eno Hall, Princeton, N. J. Edited by W. S. Hunter. Subscription $6.00. Monthly. 700 pages annually. Founded 1927.

Journal of General Psychology—Worcester, Mass. Clark University Press. Subscription $7.00. 600–700 pages annually. Edited by Carl Murchison. Quarterly. Experimental, theoretical, clinical, and historical psychology. Founded 1927.

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